Lumleian Lectures

ON LIFE AND ON VITAL ACTION IN HEALTH AND DISEASE.

Delivered at the Royal College of Physicians,

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LECTURE II.—Part I.

OF THE CONSTITUTION OF LIVING MATTER.

This most difficult problem has been but very little considered, probably because the absolute distinction between the living and the non-living matter of a living body has not been recognised. Indeed, authorities who have written very recently evidently do not admit that the distinction I have drawn between living matter and formed material exists in nature.

Feeling confident that ere long the remarkable characteristics of living matter, and its distinction from all formed matter, will be admitted, I shall venture to speculate upon the constitution of the bioplasm in the living state. In the first place, I would remark that all bioplasm contains moisture. Water is necessary to its constitution, and is invariably present in every kind of living matter, and in every state in which living matter can exist. As I have before intimated, movement of particles from centres is involved in my idea of life. Each movement would be impossible in the absence of water. The moment a particle of bioplasm is deprived of its water it must therefore die. In cases of resuscitation after desiccation the bioplasm has not been perfectly dead—has not been entirely deprived of its water.

Water must be very intimately associated with the particles of matter that live. These living particles cannot be regarded as dry solids suspended in water and moving freely amongst one another in the fluid, for, as already intimated, every particle is composed of smaller particles which are likewise in constant motion.

Now let us, guided by observation, try to realise what must be going on in every centre of the most minute of the multitudes of living centres which constitute the most minute living particle. And in the first place it is necessary to bear in mind that each central point, when magnified, seems to have circumference as well as centre. We may follow on, in our imagination, nevertheless keeping true to the line of observation, and pass into the region of speculation, and still we shall find it impossible to reach in conception a true centre. One may imagine excessively minute compound semi-fluid molecules of pabulum moving in fluid and coming within the sphere of the operation of molecules of living matter; we may conceive the lifeless passing among the very minute living particles; but to conceive how the atoms of the particles of lifeless pabulum separate from one another, and then become rearranged and acquire new living powers like those which existed before them in a living state, is not yet possible. There must be transference of power from the living to the non-living, the latter gaining power from particles which have imparted that power without having lost or created anything. In fact, a vast amount of non-living matter may acquire living property or power in a very short period from an extremely minute particle of matter already in a living state. There seems no limit to the capacity or power of making alive; no loss being occasioned by the repetition of the process, no failure or exhaustion.

Such then I believe to be the nature of the wonderful changes which occur in the minute particles of every form of bioplasm in nature, from that of the bacterium to that of man. Not only do they characterise the bioplasm of health, but that of every disease-carrying particle, which has resulted by descent from the normal bioplasm of man or the higher animals, as long as its vitality and active powers exist. Between living particles and globules of oil and fragments of albumen or any non-living matter whatever, there cannot, therefore, be the remotest analogy.

That new centres of living matter appear in pre-existing living matter is a fact that has been proved by ob[illegible]. The so-called nuclei arise in pre-existing living matter, and nucleoli in the nuclei. In the new centres themselves a second series of new centres may appear, and in these a third series, and so on; and, what is still more remarkable is, that the properties or formative powers of these different series, and of the bioplasts resulting from their division and subdivision, may be different. The new power seems to be acquired by the matter at the time when, or just before, it comes into being as a new centre. The centres in question from which matter inherits vital properties and acquires the power of transmitting these onwards, must be extremely minute. Indeed, as to the real size of the most minute particles that are made to live, one hardly dares offer a suggestion more definite than the following: that on the one hand, they must be hundreds of times more minute than the smallest particle visible under an amplification equal to five thousand diameters; and, on the other hand, they must be larger, or at least heavier, than the ultimate atoms of their component non-metallic elements. It is remarkable that those who have spoken of *living atoms* should have thought so little about the matter as to have permitted themselves to suggest an absolute impossibility, for it is obvious that *no single atom can be thought of as alive*. The idea of a living atom of oxygen or hydrogen, or carbon or nitrogen, is clearly untenable. We might as well talk of a living atom of sulphur, or iron, or lead. And it is absurd to talk of *dead* carbon, oxygen, or hydrogen atoms, since a living state of the atoms of these and other bodies is thereby implied; but such living state is inconceivable. Every form of matter that is alive is composed of several elements, and it is probable that the most minute particle of matter that lives consists of multitudes of atoms of several different substances in a state of collocation that cannot be compared with any other state of combination that is known.

But it is not merely the power of moving from centres, of progressive movement, of transforming pabulum, of forming new and peculiar chemical compounds that is acquired, but powers far more wonderful than these, by virtue of which the living matter produces living particles, generation after generation, from which definite organisms and definite and very different tissues in each single organism result. These powers are peculiar to the living world, and there are no forces, no behaviour of non-living matter, which can be said in any way to resemble them, or to exhibit the most distant analogy to them. How these powers are acquired it is too soon to speculate, but I should, nevertheless, like to refer now to one or two points. First, I would remark that time exerts an important influence in the transmission of powers from living matter to that which descends. The time required for the production of a given result, though constant within certain limits for forms of life of the same kind, varies greatly with respect to different kinds of living things. Precisely corresponding changes take place within very different periods of time, and the influence of temperature, food, and external conditions is uniform only in the case of the same species. One particle of living matter much less than the [illegible] may take weeks or months to acquire its full powers, another only a few days. We have not at present the data upon which to found general deductions, and we must withstand the temptation of framing laws and applying them to vital phenomena, until a good deal more shall be known than we have yet been able to discover, or assuredly the exceptions to the laws laid down will multiply faster than the exemplars.

Powers and properties have been attributed to pabulum, and some have said that the organism is only what it feeds upon. But what the organism is depends far more upon what the organism was that produced it than upon the particular material that constituted its food.

The pabulum of the tissues of complex animals is undoubtedly peculiar, and must be properly prepared before it can be taken up; but by what means is the preparation effected? The bioplasm of the tissues selects pabulum out of blood. But how is the blood itself formed? By bioplasm. Living matter takes up pabulum of the proper kind and grows, at last becoming resolved in formed matters, which constitute the pabulum of other bioplasm, and this process occurs several times before our food becomes converted into

our tissues. Whatever act of life we investigate we soon trace the phenomena back to bioplasm, and then we find ourselves face to face with the question of the nature and mode of working of vital power.

If the method by which non-living matter is converted into living matter is understood, by all means let it be explained. If conversion like that effected by living matter can be carried out in the laboratory let it be done; but if the change can be effected by living matter alone, let this be openly admitted, and let it be clearly stated, and in the most public manner possible, that the phenomena in question are peculiar to living matter, and cannot be shown to be due to physical and chemical changes apart from life.

By admitting vital power I am able to explain results without attributing metabolic influences to cell-walls, cell contents, intercellular substance, walls of vessels, and other textures, which, in reality, are passive. I need not assume hypothetical actions and differentiations, or attribute to some hypothetical force, said to be akin to aggregation and crystallisation, phenomena which have not the faintest analogy with those processes, nor have I need to assume governing powers of which the mind cannot conceive, or matter-guiding forces acting in some unexplained way through all sorts of matter. *Vitality* acts in living centres upon matter only infinitely near the centre. This is all that is demanded by the terms of my hypothesis. I venture to think that few who consider the facts of the case will be indisposed to grant me thus much.

ORIGIN OF LIFE.

The far-fetched conjectures seriously advanced by some physical speculators concerning the origin of life serve to show what extreme difficulty has been experienced by those who have attempted to construct a plausible hypothesis by which the conversion of the non-living into the living might be reasonably accounted for. One great authority, dissatisfied with every suggestion, and being evidently convinced that no physical explanation of the origin of life upon our globe would ever be discovered, despairingly submits to us the proposition that life did not begin here at all, and that our earth was first peopled by the offspring of germs brought to us upon a fragment broken off from some distant orb that teemed with life. Whether even the simplest living forms would have survived after such a ride through space unfortunately had not been determined by experiment, so the idea of our fauna and flora being derived from those of another world found little favour, and probably all who have considered the subject would now agree that it is probable that life-forms originated upon our globe, though there might be great difference of opinion concerning the precise mode of their origin.

"Evolution" is now supposed to solve the difficulty of life formation; but this term has had at least two meanings assigned to it. By some it has been restricted to the living world, while others have given to the term "evolution" a much wider signification, and have maintained that it should include not only the evolution of living forms from pre-existing living forms, but the formation of the living out of the non-living. There is, it is scarcely necessary to point out, the widest possible difference between these two doctrines, for while the one teaches that all living forms came direct from living matter without accounting for the origin of life at all; the other is a tenet of the fiery-cloud philosophy which teaches as a cardinal point that the evolution of life is but one of the great series of changes in which the evolution of the cosmos is comprised. But surely such an idea may, for the present, be regarded as a conjecture so extravagant as to be unworthy of serious consideration. Facts are wanting, and the arguments advanced in favour of the hypothesis are such as cannot have much weight, since it has been deemed necessary to bring forward, in their support, utterances of a prophetic character.

If, then, evolution is restricted to the living world, the origin of the first living thing will be still unaccounted for. The presence of a very simple living form seems to have been assumed; but whether that being came of itself from the non-living, or arose in consequence of some prior changes, or was formed by an act of creative interference, is not suggested by the terms of the particular form of the hypothesis under consideration. Neither is the precise nature of the first living substance indicated, and we are even left in doubt whether one or two or many forms of living matter came into existence at the first formation of life.

Now with reference to the origin of the first living matter several not improbable suggestions present themselves to the mind, in all of which, however, it is assumed that the change from the non-living to the living was sudden and abrupt, and not gradual.

First, we may conceive that one form of living matter was produced direct from the non-living, and that from this all future living was evolved.

Secondly, we may prefer to imagine that more than one form of life originated from the non-living at or about the same time.

Thirdly, we might think it more in accordance with facts to conceive that several different kinds of bioplasm originated in the beginning of an epoch of life, from which all life of that epoch was derived. New forms originating anew in the next epoch, the results of evolution from the first gradually dying out as those of the second epoch increased and became dominant. As life-epoch succeeded epoch, new forms of bioplasm may have appeared as old forms of life died out.

But the above by no means exhaust the list of what I would term the reasonable hypotheses concerning the origin of life that may at once be suggested. All of them involve in some form or other the admission of a remarkable change in capacity or power not to be accounted for by physics. In all, the communication to matter of powers or forces which it did not always possess, and which it is conceivable might never have been communicated at all, is suggested.

Whether this communication of new powers occurred once only or was repeated at many successive periods in the remote past,—whether it be reasonable to consider a recurrence of the process in the future as probable or improbable, I shall not now venture to discuss.

What I particularly wish we should keep before our minds is that facts and arguments render it much more probable that the passage from the non-living to the living is *sudden and abrupt, than that there is a gradual transition or scarcely perceptible gradation from one state to the other.* I should, however, clearly state that this inference is in opposition to the views of many authorities, and in particular is opposed to the clearly expressed opinion of one of the greatest discoverers and most acute thinkers of our time, who maintains that the conversion of physical into vital modes of force is continually taking place. It is suggested that the change from non-living matter to living matter is a transition easily effected and continually occurring. Of the facts in support of so startling a proposition I confess I am ignorant, nor have I succeeded in my efforts to discover any facts in the writings of those who appear to have accepted the conclusion in question, which has never failed to enlist advocates in its support from the time when it was believed that highly complex living forms were produced from earth or dew, to the present day, when the advocates of the doctrine are so terribly restricted in the discovery of parentless living particles.

We have now reached the point where we are brought face to face with the modern developments of the old doctrine of spontaneous generation.

I cannot but remark that the more minutely investigation is carried out—the more thoroughly and intently facts bearing upon the matter are examined—the more improbable, in my judgment, does it appear that any living form should be derived direct from the non-living. Notwithstanding all that has been recently written upon this subject, I cannot but feel surprised that at this time many good reasoners should decide in favour of the *de novo* origin even of bacteria. Whether we consider the matter from the experimental side only, or study the evidence obtained in a general survey of nature, or carefully reflect upon the facts learnt from investigations concerning the properties of living and non-living matter, with the aid of the most perfect instruments of minute research now at command, or from other standpoints, the conclusion seems to me irresistible that the verdict of a jury of well-educated men would be against the direct origin of any form of living from any form of non-living.

Driven from one position to another, the advocates of spontaneous generation have entrenched themselves in the unassailable stronghold of experimental investigation. Here

they may hold their own for any length of time, for no one can say what may not be demonstrated by new experiments in the time to come. Nay, although the conflicting results of different skilled experimenters, whose experiments have been conducted upon the same principles and professedly in the same way, even to the minutest details, may shake the confidence of some in the experimental method of inquiry, it is certain that the teachings of experiment will finally prevail over all other information.

But the modern advocate of abiogenesis should be skilled not only in explaining facts, but in explaining facts away. The fact that bacteria germs exist in all parts of the higher organisms, in the most internal parts as well as upon the surface of man's body, is to be accounted for by their spontaneous origin! Although millions are to be found about the mouth and upon the surface, and it can be shown that it is easy enough for them to get from the outside amongst the tissues within, we are asked to believe that those inside originated there direct from the non-living, or, as an alternative proposition, that they were derived not from parental bacteria, but by transmutation from some of the constituents of the tissues, on the principle that a living fungus comes not from a fungus germ, but from a dying tree. The next suggestion will be that man, after all, is but an aggregation of lower forms, peculiarly conditioned for a time, but which assume their ordinary forms when their environment shall be modified, as it must be at death.

Erroneous conclusions of many kinds have been employed as facts in support of abiogenesis. When one finds that it is believed that fungi may be developed from oil-globules and other living organisms of a much higher type, produced without parents out of organic matter, one fails to see any limit to the support that may be gained to the cause. Volumes of facts and arguments hitherto advanced in favour of abiogenesis may be republished without in the slightest degree modifying the real state of the case. What is now required is well-devised experiment, and that is all. No resuscitation of old arguments and doubtful facts, however ably the task is performed, will in the slightest degree increase the cogency of experimental proof, and in the absence of new experiment such facts and arguments will avail nothing.

I think we may be satisfied that before long the advocates of spontaneous generation will have to rely upon the production of the lowest organisms only. The only view in any way tenable at this time is, that such organisms as bacteria are the only ones that can, under any arrangement of conditions possible to an experimental inquirer, be formed anew, and that these alone, at any period of the world's history, sprang direct from the non-living. All are of extreme minuteness, many of the forms being so very small that they could not be identified with a magnifying power of less than eight hundred diameters. These are the smallest, simplest, and probably lowest forms of life known. That multitudes do now spring from pre-existing forms is absolutely certain, for the process can be seen. Whether some spring direct from the non-living is the question. Those that are supposed to be formed anew are very like those that have had a progenitor, and from those supposed to have been produced anew, forms exactly like those derived from undoubtedly pre-existing forms result. It cannot be pretended that new forms of existence are produced anew. No matter how the conditions are varied, the living forms supposed to result resemble known living forms, and give rise to forms of the same kind.

But, as I have before remarked, the question of the origin of bacteria can be only determined by experiment. All irrelevant considerations in favour of abiogenesis ought now to be left in abeyance. The assumed *de novo* origin is contrary to what goes on throughout the whole kingdom of nature, and the only exception which there is the remotest possibility of establishing is the spontaneous origin of some of these lower forms of life. While, therefore, it is allowable to permit ourselves to be influenced by general evidence against a new and exceptional doctrine, which a few observers seem very anxious to establish, we may fairly insist that only evidence of the most convincing and demonstrative kind should be accepted in its support. As regards the validity and reliability of the most recent experiments for and against the doctrine, I offer no opinion. Time must be allowed for others to repeat the experiments; and, for my own part, I could express no opinion unless I had been present and had carefully watched each experiment in every stage. As far as I can judge, the reports of recent results are not more convincing than were those that were adduced years ago, many of which have been discarded and proved to have been unreliable from want of care, or from defects in the method of procedure.

If the formation of a bacterium germ, direct from non-living matter, be possible, three very remarkable series of changes, as it seems to me, will have to be brought about. Whether any means will ever be discovered of effecting these changes is surely most doubtful.

First, the atoms of the non-living substances must be separated from their combinations.

Secondly, the atoms will have to be rearranged to constitute groups of which the organic matter is made up.

Thirdly, the groups of atoms must be made to live.

What facts known, I would ask, render it likely that air, rarefied or condensed, or pressure of any degree or of any special kind, or any degree of heat, or light, or any conceivable modification of physical or chemical conditions, would, at the same time account for the pulling asunder and joining together of atoms, and for the conference of new and peculiar powers of growth, of movement, of division, and the formation of new substances? In short, it is not easy to conceive, in the imagination, the several steps which result in the formation of a living bacterium even from *organic matter*. But the first germ must have sprung direct from matter that never had lived nor manifested phenomena in any way like those of life. Let us try to imagine a living germ being produced out of non-living matter. Atoms of many substances must be conceived as separating from one another, and then recombining. Attractions and affinities must, in the first place, be overcome, then the forces that effected the change must cease to operate; and these must, somehow, be exerted again. By what means the separation of atoms is effected cannot be suggested, neither can we conceive how the atoms are caused to recombine in a definite way. The supposed phenomena would be really more complicated than I have represented; for atoms are not related to one another—atom to atom, but group to group. How the atoms are grouped, and how the groups are related; how the groups act and react upon one another, and new groups are formed; what makes the atoms combine and begin a new course which may continue on and on for ever,—cannot be conceived. Upon the whole the production from non-living matter of any living form, however simple, must be regarded as most improbable.

Lettsomian Lectures

ON THE

SURGERY OF THE ARTERIES.

(ILLUSTRATED BY TWENTY-SEVEN OPERATIONS OF LIGATURE BY THE AUTHOR, AND OTHER CASES OF DISEASE AND INJURY.)

Delivered before the Medical Society of London, Jan. 1875,

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LECTURE II.—PART II.

THE LIGATURE.

OF the various substances, whether animal, vegetable, or mineral, which have from time to time been employed for the ligature of arteries in their continuity, silk has been the most highly approved. But the fact that trials of other substances have been made is an indication that something is wanting even in silk.

To secure the obliteration of an artery, a sufficiently strong, fine, round and firm thread is used, and tied tight, with a view to cut through, and maintain in apposition, the two inner coats of the vessel and to strangulate the outer. Thus the ligature, employed as a curative agent, at once inflicts an injury which not unfrequently has a fatal result. To be successful, it must give rise both to a *reparative* pro-

cess in the shape of *adhesion* of the cut surfaces of the divided coats, and to a *destructive* process in the shape of *ulceration*, by which the ligature is to be cast loose, holding sometimes in its grasp a small slough of the constricted outer coat of the vessel. Should adhesion fail, or ulceration exceed certain limits, the operation may be destructive to the patient. Independently of the above processes, the formation of a coagulum in the vessel on both sides of the ligature is a safeguard against hæmorrhage, although not essential to prevent it when firm adhesion has occurred. It should, however, be encouraged; and with that object in view the surgeon elects, when he can do so, to place his ligature at a distance of not less than an inch and a half from a known branch. He does this because observation has shown that the proximity of a branch to the ligature through which former blood is rapidly passing is inconsistent with clotting, as is shown also by one of my cases. The brachial artery was tied, and the patient died after apparent cure, as in Mr. Spence's case, where flexion afterwards succeeded.

The operation performed by a ligature upon an artery corresponds as nearly as possible, so far as the vessel itself is concerned, to a *subcutaneous* operation, the two inner coats being severed under cover of the outer, and possibly this fact it is which often favours adhesion, even though the injury inflicted be a *contused* wound. But, added to this concealed injury we have strangulation of the external coat, and its attendant consequence—ulceration. Herein lies the weak point of the ligature. Now and then an artery is cut down upon, and a ligature of silk, or gut, or of wire, or of catgut, having been applied and cut short, the wound closes by primary union over the foreign body, but this result of the operation cannot be relied upon.

With the hope of obviating the casting off by ulceration of the ligature, Dr. Jones* about seventy years ago, made experiments on horses and dogs, and showed that after

TABLE OF MR. MAUNDER'S NINE CASES OF LIGATURE OF ARTERIES IN THEIR CONTINUITY ON THE ANTISEPTIC CARBOLIC METHODS.

Case.	Sex.	Age.	Nature of disease or injury.	Operation.	Result.	Remarks.
{1* {2	M.	37	Aortic aneurism (supposed innominate)	Ligature of common carotid and of third part of subclavian simultaneously, Sept. 18th, 1867	Died on the sixth day	Antiseptic silk ligature, long, was used; no suppuration.
3	M.	46	Axillary aneurism	Ligature of subclavian, third part, March 18th, 1871	Died on the ninth day	Antiseptic catgut ligature, cut short, was used, and is still on the artery; decomposition occurred in the wound; diffuse cellulitis; pericarditis.
5	M.	26	Carotid aneurism at bifurcation, left side	Ligature of common carotid, Sept. 22nd, 1868	Recovered	Antiseptic silk ligature, cut short; healed in a few days without suppuration. Ligature still on artery (Jan. 1875).
6	M.	32	Femoro-popliteal aneurism	Ligature of superficial femoral, May 17th, 1872	Recovered	Wound healed by primary union over antiseptic catgut, cut short.
7	M.	42	Popliteal aneurism	Ligature of superficial femoral, June 16th, 1872	Recovered	Antiseptic catgut ligature, cut short; wound quickly suppurated.
12	F.	3½	Open knee-joint	Ligature of superfi. femoral, April 29th, 1869	Recovered	Fishing-gut antiseptic ligature, cut short; no suppuration for several days.
14	M.	31	Wound of wrist; secondary hæmorrhage	Ligature of brachial artery, Nov. 16th, 1871	Recovered	Antiseptic catgut, cut short.
17	M.	28	Suppuration of hand and secondary hæmorrhage	Ligature of brachial artery, March 11th, 1873	Recovered	Antiseptic catgut, cut short. Died of pyæmia on the eleventh day. No ligature to be found. Inner coats of artery divided and united by adhesion.

* Two arteries tied.

(The numbers correspond with Mr. Maunder's Table of Arteries published in THE LANCET, Jan. 9th, 1875, p. 39.)

on the eleventh day of pyæmia. Union of the divided coats had taken place, but there was clot on the distal side only. On the proximal side a vessel came off near to the seat of ligature. Lister and Callender have suggested that one reason why coagulation takes place more readily on the cardiac than on the distal side of a ligature is because of the turmoil into which the blood is thrown when impelled with force against an obstacle. At the same time a certain length of vessel between the next branch and the ligature, in which the blood may be whipped up and yet be uninfluenced by the current, is requisite. The above case illustrates these points, because, although a small clot had formed on the distal side of the ligature, and the blood might be expected to be in a state of comparative repose there, yet, knowing how free is the anastomosis in the upper extremity, it probably was, on the contrary, much agitated. And this free anastomosis, by promoting the deposit of a tolerably firm clot on the distal side of the ligature in the upper extremity, is one reason why secondary hæmorrhage is comparatively rare in this region of the body. This churning process leads to the deposit of fibrine more or less pure in proportion to the disturbance, and thus a firm obstacle to secondary hæmorrhage is presented above the ligature. On the other hand, we cannot ignore the apparent fact that the formation of *blood-clot*, in contradistinction to pure fibrine, is favoured by slowness of movement of the fluid holding it in suspension. Consequently, where the anastomosis is not very free, as in the lower extremity, the deposit of soft blood-clot rather than of fibrine may be expected on the distal side of the ligature. Supposing, then, from any cause a small aperture to exist in the vessel on the distal side, and before this soft clot shall have become consolidated, the force of the blood propelled against it may be sufficient to wash it away, and so secondary hæmorrhage occurs. The instability of soft clot is recognised in some cases of aneurism cured by the rapid method of compression and by ligature, in which the circulation is re-established tying two or three ligatures tight upon an artery, and immediately withdrawing them, obliteration of the vessel at the seat of injury occurred, without destruction of the outer coat, and consequently without loss of continuity.

Mr. Lister's experiment on a calf is also instructive. He tied the carotid artery of a calf *antiseptically* with an animal ligature, and at a post-mortem examination, made thirty days after, found it embracing the artery and organised.

The information thus derived by these two experimenters would be of the utmost value were we called upon to treat the lower animals, but until we know by actual practice on man what the result of operations performed in a similar manner is, no useful conclusions can be reached. Webber's forceps, intended to sever the inner coats, and then to be withdrawn and the wound closed, though intended to act on the same principle as what I may term Jones's *momentary* ligature, has the disadvantage of dividing the inner coats at two points, and including and compressing between these about one-sixth of an inch of the calibre of the vessel.

Theoretically, the chief risk which the use of either Jones's momentary ligature or Webber's forceps would entail would be secondary hæmorrhage, supposing the bruised external coat to slough before the channel of the artery had become obstructed by adhesion. But if, in man, *adhesion* and obliteration, while blood is traversing the injured part of the vessel, is not a necessary consequence, and *sloughing* is a necessary result of the method employed, the effect will be disastrous.† A temporary ligature applied for a few hours only, and then removed, was tried by Sir Astley Cooper and others, and yielded the most unsatisfactory results. Our great aim should be, out of a variety of means to select that which commends itself at first theoretically, and then stands the test of experience.

I shall now consider the last novelty — the antiseptic method. It is not necessary to inquire here how this anti-

* Dr. J. F. D. Jones on Hæmorrhage, 1805.

† L. Holden, Bartholomew's Hospital Reports, vol. viii., p. 187.

septic method operates. All will accept that it is desirable to prevent putrefaction and to promote rapid closure of wounds. To this end carbolic acid is used, and catgut, *properly prepared*, is advocated by Mr. Lister to replace the time-honoured silk thread.*

Since the introduction of the antiseptic ligature I have resorted to it on eight occasions, using various materials, sometimes cut short, at others left projecting at the wound.

No. 1.—In Case 12, that of a child whose femoral artery I ligatured to arrest traumatic inflammation associated with an opened knee-joint, I employed fishing gut, protecting the wound with dressings of carbolic oil and lint. The ligature was cut short, and for several days there was no secretion from the wound beyond serous oozing, and probably its deeper parts healed quickly, although the surface after a time secreted pus.

No. 2.—In Case 5, that of a gentleman whose left common carotid artery I ligatured, dentist's silk, soaked previously for some hours in a very strong solution of carbolic acid, was used and cut short. The wound from time to time during the operation was flooded with a solution of carbolic acid, and after the sutures had been introduced it was covered up with carbolic putty. A little serous oozing alone occurred, and within a fortnight the wound was soundly healed, and remains so to this day.

No. 3.—Case 1, that of a man whose carotid and subclavian (third part) were tied simultaneously with carbolised silk left projecting at the wounds. The patient died on the sixth day.† No suppuration had occurred.

No. 4.—Case 3, a male whose subclavian I tied antiseptically with catgut cut short. Decomposition occurred in the wound. The patient died of diffuse cellulitis and of pericarditis. In the preparation† the ligature still embraces the artery.

No. 5.—In Case 14 the brachial artery was ligatured for secondary hæmorrhage from the wrist with catgut, cut short. The wound remained open superficially an unusually long time. The man was very feeble from loss of blood.

No. 6.—In Case 17 the brachial artery was ligatured with catgut, cut short, for hæmorrhage from the hand associated with suppuration and sloughing. The patient died of pyæmia on the eleventh day. At a post-mortem examination no ligature could be found, but the coats of the artery were adherent at the point of section by it.

No. 7.—Case 6: Catgut ligature (cut short) of the superficial femoral artery for popliteal aneurism. The wound was soundly healed in a week.

No. 8.—Case 7: A catgut ligature, cut short, to the superficial femoral under precisely similar circumstances to the former case. This wound quickly suppurated, and remained open many weeks.

I have also witnessed two other instances in which catgut was employed antiseptically. One, that of my colleague Mr. Couper. The patient was the subject of carotid aneurism, which extended low down into the neck. The operation was necessarily a difficult one, and a great deal of time, trouble, and manipulation were expended before the artery could be reached; and this close to the sac, which was both seen and touched. I thought at the time that diffuse cellulitis would certainly follow, but, on the contrary, I was agreeably surprised to find that, with the exception of a very little serous oozing, the wound was healed in about a week. It certainly was a striking instance of rapid healing under apparently very adverse circumstances.

The other case, that of Mr. McCarthy mentioned in my former lecture, was one of simultaneous ligature of the subclavian and common carotid. Catgut, cut short, was used antiseptically. Decomposition occurred in the subclavian wound. The patient died of hæmorrhage from the cardiac side of the subclavian ligature on the fifteenth day. At the post-mortem examination the ligature was found on the subclavian artery, the latter having a small hole in it corresponding in position to the knot. It had not divided the inner coats. The carotid wound had nearly healed. No ligature could be found here, but a considerable quantity of thickened connective tissue had been developed at the seat of ligature. The coats of the artery were not divided, and there was a little clot at both the proximal and the distal side of the ligature. The aneurism was aortic.

Of these ten cases two died (No. 3 of mine, and Mr. McCarthy's) of causes occasionally associated with the application of a ligature to the third part of the subclavian artery—hæmorrhage and diffuse cellulitis,—the latter giving rise to pericarditis and pleuritis. These two are the only cases in which death may be fairly ascribed directly to the ligature.

No. 17 of mine also died, but of pyæmia, and although no ligature was found upon the artery, repair was taking place in it, notwithstanding the unfavourable general condition of the patient.

Nos. 1 and 2 of mine also died, but not from the direct application of the ligatures to the carotid and subclavian. The wounds remained perfectly antiseptic, and had the patient survived, it may be fairly assumed from that fact, and also from the post-mortem appearances, that the threads would have come away favourably.

Nos. 15 and 16 were a long time in hospital before the operation wounds healed, and it may be fairly suggested that a more favourable set of symptoms would not have followed the application of the silk ligature in the usual way, by reason of previous loss of blood.

Nos. 6 and 5 are examples of rapid healing, while the ligature, in the one case silk, in the other catgut, remained on the artery. No more satisfactory result could be desired.

In contemplating this list of cases with a view to determine the value of the antiseptic method of ligature, the question to be asked is: Could an equally or more favourable result have been anticipated had the usual operation with the silk ligature been performed? My reply will be in the negative.

* THE LANCET, April 3rd, 1869. † London Hospital Museum.

An Address
ON
THE GERM THEORY OF DISEASE;
BEING
A DISCUSSION OF THE RELATION OF BACTERIA AND ALLIED ORGANISMS TO VIRULENT INFLAMMATIONS AND SPECIFIC CONTAGIOUS FEVERS.

Delivered before the Pathological Society of London, April 6th, 1875,

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WHEN honoured by a request from the Council of this Society, a few weeks since, to open a debate during the current session, compliance with such a wish was regarded by me as a professional duty. I was compelled, therefore, to do the best I could with the short time and limited leisure which presented themselves, though these, I regret to say, have proved insufficient to enable me to bestow the attention I should have desired upon the vast accumulation of writings directly or indirectly related to the subject selected for discussion, and quite insufficient also to enable me to throw light upon it, to the extent I should have wished, by certain new observations of my own. The subject, however, large as it is—and consequently difficult to be dealt with satisfactorily in the space of one hour,—seemed to recommend itself for several reasons:—(1) It is a question lying at the root of the pathology of the most important and most fatal class of diseases to which the human race is liable—diseases which cause nearly one-fourth of the total number of deaths in this country. (2) It is a subject important alike to those engaged in almost every department of our profession. And (3) it is one which I happen to have very carefully considered for several years, and for the elucidation of which I was tempted in 1869 to undertake long and laborious investigations—though these may have seemed to many to have little practical bearing upon the science of medicine.

The subject of the relation of the lower organisms to disease has, moreover, a growing importance. The notion that there is a distinct causal relation between the two—though it has long existed in one form or another—is one which has

been spread enormously within the last few years, partly owing to our increase of knowledge concerning these low organisms, and partly because of their ascertained presence in numerous diseased tissues and exudations. Medical literature both at home and abroad now, in fact, teems with papers and memoirs bearing upon this relation, and such communications rapidly increase in number year by year.

In the short time allotted to me to open the debate, I shall be able to make specific allusions to but few of these contributions, as it would seem better to keep the broad issues well in view in my opening statement, and reserve questions of detail, as these may be taken up by other speakers and subsequently commented upon if necessary.

The one common and distinguishing feature peculiar to all the diseases whose pathology we are now about to consider is their "contagiousness." An individual affected by either of them throws off particles from the region specially affected, or from many parts of the body, and these particles, on coming into contact with suitable surfaces in other persons, may incite similar local or general diseases—though such results do not invariably follow. This peculiarity, by means of which such diseases are spread amongst the members of a community, was, even in the time of Hippocrates, compared to the property by which one fermenting mass may communicate its state of change to another mass of fermentable material. Throughout all intervening periods such an analogy has never been lost sight of—it has rather been more and more strongly dwelt upon. Thus, more than two centuries ago, we find, as has been recently pointed out, Robert Boyle, one of our great English philosophers, and himself a pioneer in scientific investigation, giving strong expression to the then current view: "He that thoroughly understands," he says, "the nature of ferments and fermentations shall probably be much better able than he that ignores them to give a fair account of several diseases (as well fevers as others) which will perhaps be never thoroughly understood without an insight into the doctrine of fermentation." Again, in more recent times, it was doubtless under the influence of a belief in the same analogy between fermentations and the class of diseases of which I am about to speak that the term "zymotic" was proposed by Dr. Wm. Farr and adopted as a general designation under which nearly all these diseases might be included. The consequence of the adoption of this nomenclature has been that views as to the nature of the infecting something, or *contagium*, have since been so powerfully influenced as to be actually led by views at the time entertained concerning the nature of *ferments*—the relationship supposed to exist between zymosis and fermentation has indeed been stamped and ratified by the very general consent of the profession.

Omitting for the present any remarks as to the real strength of this analogy, I would merely further point out that the foundations of the "germ theory of disease," in its most commonly accepted form, were laid in 1836 and shortly afterwards. The discovery at this time of the yeast plant by Schwann and Cagniard-Latour soon led to the more general recognition of the almost constant association of certain low organisms with the different kinds of fermentations. But it was not till twenty years afterwards that Pasteur announced, as the result of his apparently conclusive researches, that low organisms acted as the invariable causes of fermentations and putrefactions—that these, in fact, though chemical processes, were only capable of being initiated by the agency of living units. If, in accordance with this somewhat narrow and exclusive view, living units were to be regarded as the sole producers of fermentation and putrefaction, then they were *sole* ferments. The extension of this doctrine by medical men to contagious diseases, in face of the analogy sanctioned by the use of the term "zymotic," became only too easy. It was obviously nothing but the logical outcome of the two sets of views to hold that low organisms were the true contagia, or sole "germs" of the so-called zymotic diseases.

It so happens, therefore, that the very exclusive notion just mentioned as to the nature of contagia is at present almost as deeply rooted in the minds of the majority of writers on epidemic diseases and contagious fevers as was the opposite notion, founded upon the physico-chemical doctrine of Liebig, some twenty years ago. Then a ferment was regarded as a portion of organic matter (not necessarily living) in a state of molecular change ("motor decay"), which, by virtue of its own unstable nature, was capable of communicating molecular movement (chemical change) to other unstable or fermentable mixtures. This broader notion was promulgated by Liebig at a time when less was known than at present as to the constant association of low organisms with the processes of fermentation and putrefaction. The nature of this relationship was, in fact, never adequately grappled with by him. Still, views of the kind promulgated by Liebig would not give anything like the same support to the germ theory of disease as that afforded by the doctrines of Pasteur. Those who have adopted and developed Liebig's views now hold that living organisms, though they may operate as ferments, act in this capacity merely by virtue of the chemical changes which the carrying on of their growth necessitates; and that other chemical changes taking place during the decay of organic matter may make fragments of it (in the dead state) almost equally capable of initiating fermentative changes in suitable media; whilst in either case bacteria or allied organisms are prone to be engendered as correlative products.

In the present day, therefore, two questions seem to need the serious consideration of medical men. In the first place, it may be asked, Are we justified in relying so strongly upon the analogy between fermentation and zymosis? secondly, we may inquire whether the researches by which Pasteur claims to have established the sole nature of ferments are so conclusive as they have been commonly regarded? In reply to the first question, certain qualifying considerations will hereafter be stated, though it may be at once admitted that the analogy is so strong as to make it likely to continue to exercise a very considerable influence upon medical opinion. It therefore becomes all the more necessary for medical men to look to the foundations of Pasteur's doctrine, if they are not prepared blindly to follow his dicta on a subject which is of so much importance for medical science. It was with this view that I undertook, a few years ago, and shortly after I had been called upon to teach pathology, a series of investigations bearing upon this subject. In consequence of this work I was compelled, as others had been, to refuse assent to the exclusive doctrines of Pasteur concerning the nature of ferments. I do not enter upon this discussion now. I maintain, however, that my own investigations and those of others show that units of living matter are not sole ferments—since fermentation and putrefaction may be initiated in their absence, and since it can be shown that mere particles or fragments of organic matter may act in this capacity. For a brief exposition of the grounds of this belief I would refer those interested in the matter to my recently published work, "Evolution and the Origin of Life."

Some time must be allowed to elapse before anything approaching to general agreement can be expected on such a subject; and, meanwhile, standing as we do in the face of opposite doctrines as to the nature of ferments, we are free to look into the question of the relation of the lower organisms to disease on its own merits—apart, that is, from the overweening influence of any general theory of fermentation.

Leaving on one side, therefore, the influence of the analogy deemed to exist between the process of fermentation and that of zymosis, we may ask what other general evidence is forthcoming in favour of the notion that contagia are low organisms or living units, rather than dead organic particles from altered tissue-elements, or complex chemical compounds of alkaloidal constitution engendered in the tissues or in some of the fluids of the body. The consideration of this question may be introduced by a quotation from Dr. Burdon-Sanderson's valuable report on the "Intimate Pathology of Contagion" (Twelfth Report of the Medical Officer of the Privy Council, 1870, p. 243). He says:—"There are two obvious objections which stand in the way of the acceptance of any chemical explanation of the phenomena of contagion. The first is, that the multiplication of contagium in the body of the infected individual is a process which cannot be compared to any which is brought about by chemical agencies independently of organic development. The second is, that all contagia possess the power of retaining their latent virulence for long periods (often resisting the most unfavourable chemical and physical conditions), and only show themselves to be what they are when they are brought into contact with [the] living organism. Outside of the body the contagious material withstands all those changes to which, on chemical grounds, we should expect it to be liable; while in the body it manifests

a degree of activity, and gives rise to an amount of molecular disturbance, which is quite as unaccountable. ..... Neither of these difficulties stands in our way if we suppose that the contagious process is connected with the *unfolding of organic forms.*"

Now, though this is about as strong a statement as can be made, from an *à priori* point of view, against the mere chemical action of contagium and in favour of a germ theory, I must confess that neither of the considerations seems to me to carry very much weight with it. I should be inclined to say, in reply, (1) that proof is altogether wanting of the "multiplication of contagium" in the body in the same sense that a living unit multiplies; and that there are physico-chemical processes which may illustrate what occurs when contagium increases within the system. Instead of being an increase by continuous organic development and multiplication, it may be that contagium augments by some such process as that by which crystals of sulphate of soda increase or "multiply" when a fragment of such a body is thrown into a complex fluid containing its component elements. This is confessedly a very imperfect illustration, and one to which I resort merely to indicate the possible occurrence of another mode of increase of contagium within the body; though in an infected animal such increase may occur in a much more subtle manner, owing to the fact that fluids altered, directly or indirectly, by the original contact of contagium with some part of the body are either locally or generally brought into intimate relation with the active, though modifiable, living units of the various tissues. And (2) in reply to Dr. Sanderson's other objection, which stands, as he supposes, in the way of any chemical explanation of the phenomena of contagion, I should say that, although our knowledge is at present extremely vague concerning the power possessed by the various contagia of retaining their virulence for long periods and of resisting unfavourable physical and chemical conditions, we have no reason to believe that the more complex combinations of which living matter is composed are capable of resisting influences which would prove destructive to less highly complex not-living substances—such as snake-poison, woorara, or other compounds of this class. The general evidence is therefore, as I read it, certainly not more favourable to a vital or germ theory than to a physico-chemical theory as regards the nature and action of contagia.

I should here point out, however, that under the term "germ theory" two distinct views are included, each having their advocates amongst distinguished members of this Society. The side to which Dr. Sanderson leans is sufficiently obvious. Speaking of contagious particles, he says (*loc. cit.*, p. 255): "With reference to their mode of action, we have examined into those considerations which seem to render it probable that they are *organised beings, and that their powers of producing disease are due to their organic development*; and we have accepted this doctrine as the only one which affords a satisfactory explanation of the facts of infection."*

This is the doctrine with which we are at present especially concerned, though it may be well for me to say a few words concerning the other sense in which a "germ theory of disease" is maintained by a distinguished member of this Society. Dr. Beale says ("Disease Germs, their Real Nature," 1870, p. 5): "We have therefore now to inquire what is the material substance which passes from the diseased to the healthy organism in small-pox, in measles, in scarlet fever, and other allied contagious diseases from which man and domestic animals suffer so severely. *The material in question grows and multiplies and produces its kind, as all living things do*, and as nothing that does not live has been proved to be capable of doing. We may therefore conclude that it is living matter." And as to the derivation of such matter Dr. Beale says, "a disease germ is probably a particle of living matter derived by direct descent from the living matter of man's organism," though he supposes it to be altered and degraded as regards formative power by previous rapid multiplication of the tissue elements or particles from which it has been derived. In many respects I am disposed to assent to this view, so long as it is not taken in too exclusive a sense. I will now, however, only mention what I consider to be its weakness. It seems to me that proof is wholly wanting as regards the statement which I have had printed in italics. That there is an enormous increase of germinal particles in the blood and in many of the tissues in these specific contagious diseases Dr. Beale has helped to show us by his valuable researches upon the pathology of the cattle plague and other allied affections; but that such germinal or living particles are in any direct sense the descendants of the particles which acted as contagia, or, in fact, that the contagious particles really multiply to any extent in the body,—these are propositions which at present appear to me to be wholly devoid of all proof. I and other pathologists are free to hold that contagious particles, whether composed of living or of not-living organic materials, may initiate changes in the tissues and fluids with which they come into contact, which changes may be exaggerated as they spread, so as at last to implicate the blood. And as one result of this altered constitution of the nutritive fluid and of the general febrile condition simultaneously excited, we may get that undue proliferation of tissue elements and multiplication of their products which appear to go on in the blood and in the various tissues of persons suffering from these febrile diseases. Beyond these surmises we seem also to have to do with mere conjecture rather than with established facts.

Leaving this aspect of the question, therefore, I now turn to the special subject of this debate—viz., the truth of the germ theory as it is ordinarily understood, or the relation of the lower organisms to virulent inflammations and their sequelæ on the one hand, and to specific contagious fevers on the other.

Applicability of the germ theory to virulent inflammations and their sequelæ (gonorrhœa, purulent ophthalmia, erysipelas, hospital gangrene, puerperal fever, pyæmia, septicæmia, &c.)

A few years ago no one would have thought of connecting the contagiousness of gonorrhœa or of purulent ophthalmia with the presence of bacteria. The respective secretions were known to contain some poisonous element either in the form of a chemical compound or altered product of tissue-multiplication (pus), which, when it came into contact with a healthy mucous membrane, was capable of acting as a specific irritant, and there exciting a similar morbid process. It is by no means certain, however, that some pathologists would not at the present time connect this process with the presence of bacteria in the contagious fluids. Such a point of view has, indeed, been directly fostered by doctrines recently put forward by an eminent pathologist—Dr. Burdon-Sanderson. At this Society in 1871, whilst, strangely enough, professing to be indifferent to the mode of origin of bacteria, Dr. Sanderson said: "They afford a characteristic by which we may distinguish the products of infective inflammation from those which are not infective." And in a more recent paper on "The Infective Product of Acute Inflammation" (Medico-Chirur. Trans., 1873, p. 354), referring to his previous researches, he says it was inferred from these that, "if infective agents are particulate, they are probably comprised in that group of bodies to which I then applied the term microzymes—recognising their identity with the *zooglœa* of Cohn, the *micrococci* of Hallier, and the various forms described by other authors under the terms *bacterium* and *vibrio*." And he then adds, as the result of subsequent investigations, the following passage:—"With reference to these organisms two entirely new and most important facts have been demonstrated by the observations to be now recorded. It has been discovered (1) that, in all acute infective inflammations, microzymes abound in the exudation liquids; and (2) that the same forms are to be found in the blood of the infected animals." And when Dr. Sanderson subsequently adds "that the relation of intensity between different cases of septicæmia and pyæmic infection is indicated by the number and character of these organisms," but little doubt seems to remain concerning his views as to the causal relationship of such organisms to the infectiousness of the inflammations referred to. And this view is not essentially modified by his subsequent concluding explanations, where he says: "Inasmuch as these organisms cannot have originated from the normal tissues or juices, they must have been derived from the external moisture." And, also, "it does not at all follow because these organisms come in from outside that they bring contagium along with them; for it may be readily admitted that they may serve

* These words occur in a summary which, it is only right to add, was immediately prefaced by the following statement: "The sentences which follow must therefore be accepted by the reader as nothing more than indications of the questions we are trying to solve, or as forecasts of what we hope to establish or disprove by experiment."

as carriers of infection from diseased to healthy parts, or from diseased to healthy individuals, and yet be utterly devoid of any power of themselves originating the contagium they convey." Such a doctrine still implies that bacteria are essential to a contagious process, though it seems to me to introduce certain very striking elements of weakness into the germ theory as thus interpreted. If this theory is not tenable, without the aid of some supplementary hypothesis, I cannot conceive that the introduction of the one above mentioned will be considered to have strengthened its foundations. Yet Dr. Sanderson apparently saw the difficulty of maintaining the germ theory in its integrity, and offered us this other view as a compromise. He considers it probable that, whereas true contagia, whether living particles or chemical compounds, may be engendered within the body in the tissues themselves, such contagia are not able to spread either within or outside without the aid of bacteria to act as "carriers." But why one set of particles should need others to carry them, or why bacteria alone should be able to bear about these mysterious contagious poisons which they are devoid of the power of originating, does not at all appear!

However complicated the doctrine may have been rendered, this is still practically the germ theory; and the same thing may be said with reference to a view which Professor Lister seems to entertain with some favour. He thinks that the lower fungi and their relations bacteria may contain within themselves some chemical compound absolutely peculiar to them, and forming part of their substance, which may act upon albuminous compounds after the manner of a ferment, such as emulsin. (*Nature*, July 17th, 1873.) "In this sense," he thinks, "as intervening between the growth of the organisms and the resulting decompositions, the theory of chemical ferments might be welcomed as a valuable hypothesis." This seems like the language of concession, but, practically, it is the germ theory still, and expressed too much, as all germ theorists who think out their views would have to formulate them. It would be no great concession to those who are not believers in an exclusive germ theory if, in the light of his views as above expressed, Professor Lister were to say that bacteria were "carriers of infection"; yet the apparent concession above referred to is no more of a concession to believers in a physico-chemical theory than the latter admission would be.

I will, however, now briefly enumerate the evidence which seems to me quite sufficient to disprove the probability of the existence of any causal relationship between the lower organisms and the diseases cited at the head of this section, and to establish, on the other hand, the position that the bacteria met with in diseased fluids and tissues are for the most part actual pathological products—that they are, in fact, engendered within the body, or are descendants of organisms owning such an origin, rather than of previously existing organisms introduced from without. It would take far too long were I to attempt to enter at any length upon a consideration of this evidence. I must therefore content myself with briefly summarising the principal facts and arguments on which a judgment may be founded.

1. The experiments of many investigators prove that the alleged causes of diseases may be actually introduced into the bloodvessels of lower animals by thousands without producing any deleterious effects in a large proportion of the cases.

2. Bacteria, if not actually to be found within the bloodvessels of healthy persons, do nevertheless habitually exist in so many parts of the body in every human being, and in so many of the lower animals, as to make it almost inconceivable that these organisms can be causes of disease. In support of this statement I have only to say, that even in healthy persons they may be found in myriads in and about the epithelium of the whole alimentary tract from mouth to anus; they exist throughout the air-passages, and may be found in mucus coming from the nasal cavities, as well as in that from minute bronchi. They exist abundantly amongst the epithelial débris within the ducts of the skin, not only in the face, but in other parts of the body. Fresh legions of them are also being introduced into the alimentary canal with almost every meal that is taken, whence they may perhaps readily find their way into the mesenteric glands, if not further within the system. And lastly, in persons with open wounds, bacteria are constantly to be found in contact with such surfaces, especially if the wounds are not well cared for, though the injured person does not necessarily suffer at all in general health.

3. It is no answer to these difficulties to say that there are distinct species amongst these lower organisms, some of which are harmless, though others are poisonous (or so-called "germs" of disease). In support of such opinion nothing can be alleged save some of the facts whose cause is doubtful; whilst against such an interpretation may be brought the experiments of several investigators, showing that bacteria are the creatures of circumstance, and modifiable to an extraordinary degree. The last position is even admitted by Professors Sanderson and Lister. The former acknowledges that they are "the lowest organisms," and that they are "much more under the influence of the conditions under which they originate and are developed than organisms of any other class"; whilst Professor Lister's own work has compelled him to make an admission which, in the face of facts previously stated concerning the wide distribution of bacteria within the body, seems fatal to a consistent belief in the germ theory of disease. He says: "If the same bacterium may, as a result of varied circumstances, produce in one and the same medium fermentative changes differing so widely from each other as the formation of lactic acid and that of black pigment in milk, it becomes readily conceivable that the same organism which under ordinary circumstances may be comparatively harmless may at other times generate products poisonous to the human economy." (*Quarterly Journal of Microscopical Science*, October, 1873.)

4. The consideration now to be mentioned suffices, in my opinion, to complete the discomfiture of the germ theory as an explanation of the mode of causation of the diseases with which we are at present concerned. It is this. It has been shown, on the one hand, that the virulence of certain contagious mixtures diminishes in direct proportion to the increase of bacteria therein; and on the other hand, it has been equally proved that fresh and actively contagious menstrua lose scarcely any of their contagious or poisonous properties after they have been subjected for a few minutes, when in the moist state, to a temperature which no living units can be shown to survive (212° F), or after they have been exposed to the influence of boiling alcohol, which is well known to be equally destructive to all recognised forms of living matter. Such facts have been substantiated by Messrs. Lewis and Cunningham, Sanderson, and others.

Having said thus much in opposition to the germ theory, let me as briefly enumerate the facts and arguments which seem to me to show the real relations of bacteria and their allies to the diseases in question. I turn therefore to the construction of an opposite doctrine.

Admitting in part the very frequent presence of bacteria in diseased fluids and tissues, I consider that their presence and import should be differently explained. I say I admit the association in part, though I by no means admit it to the extent alleged. Bacteria are not, for instance, to be found in the blood of persons suffering from pyæmia, as might be inferred from former statements of Dr. Sanderson which I have already quoted. My own experience in this matter seems to be entirely in accordance with that of Professors Billroth and Stricker. Neither do I believe that the presence of bacteria in inflammatory fluids has the significance which Dr. Sanderson attaches to it, since it has been ascertained by myself and others that the exudation fluids of sick persons suffering from diseases of a totally different type are often similarly crowded with these lowest organisms, whilst the recent observations of M. Bergeron (*Compt. Rend.*, Feb. 1875) seem to show that they may be found even in freshly extracted pus from ordinary abscesses occurring in elderly persons.

Now, it would seem quite obvious that the consistent advocate of a germ theory of disease can only successfully maintain such a doctrine if he can show, amongst other things, that bacteria are more capable of altering the character and chemical constitution of fluids of the body than they are themselves prone to be altered by independently initiated changes taking place in such fluids. It seems, therefore, like unintentionally cutting himself free from the theory to which he has hitherto adhered when we find Professor Lister, in speaking of the assumed "special virus of hospital gangrene," going on to say that "it is not essential

to assume the existence of a special virus at all, but that organisms common to all the sores in the ward may, for aught we know, assume specific properties in the discharges long putrefying under the dressings." This passage has a similar import to that of a quotation previously made. In both a first place is assigned to the modifying influence of altered fluids; and however much the correctness of such a supposition would tell in favour of cleanliness, free exposure, or even of antiseptic dressings, it is none the less inimical to a consistent holding of the theory on which Professor Lister has chosen to base his system of treatment.

But though such statements are adverse to the holding of a germ theory in the only form in which it may be at all tenable, they are entirely in accordance with my own observations and views. I maintain, in short, that even the very existence of organisms in the fluids and tissues of diseased persons is for the most part referable to the fact that certain changes have previously taken place (by deviations from healthy nutrition) in the constitution and vitality of such fluids and tissues, and that bacteria and allied organisms have appeared therein as pathological products—either by heterogenesis, or by what I have termed archebiosis, or birth direct from a fluid.

The evidence on which my belief is founded is of this nature:—

1. Bacteria and their allies are found in greatest abundance during the life of the individual in connexion with dying tissue elements, and apparently are as plentiful within the dying epithelium of the cutaneous ducts as in parts like the mouth, which are most liable to contamination with organisms from without. Again, they exist abundantly in and about the dying cells of bronchial mucus, although living bacteria appear to be almost completely absent from ordinary air.

2. The microscopical examination of such epithelial or mucous elements also favours the notion that the contained bacteria are products engendered within such cells rather than mere results of an external contamination and imbibition. This opinion is based upon the following considerations. Bacteria only appear within the cell when it is obviously dying, and in the case of epithelium, for instance, they manifest themselves at first as minute motionless particles scattered through the semi-solid substance of the cell, where each particle grows into a distinct bacterium, which still remains motionless, and does not appear to divide for a long time. This is precisely similar to what I have observed over and over again, when amœbæ in vegetable infusions get into an unhealthy condition and become resolved into nests of bacteria. They may exist for days in a state of activity with bacteria in the fluid around them, though none are to be seen in their interior. After a time, however, the chemical constitution of the fluid seems to become no longer suited to the amœbæ; their activity ceases, they remain as almost motionless balls of jelly, and soon multitudes of the minutest particles appear throughout their substance, each of which straightway grows into a bacterium. The former amœba is converted into a mere bag of bacteria, which after a time ruptures, and thus liberates its swarming colony of newly-engendered living units. Multitudes of mucus corpuscles seem to undergo the same kind of change, so that bacterial degeneration takes place in the same manner and is almost as typical amongst them as is fatty degeneration amongst pus-corpuscles. The two kinds of degeneration, moreover, commonly occur side by side in epithelial débris. Bacterial degeneration takes place where the vitality of the unit is lowered, but where it is not sufficiently degraded to permit of the still lower and more obviously destructive process of fatty degeneration; and if anyone wishes to see it in perfection let him examine some central portion of the kidney or other internal organ of a warm-blooded animal five days or more after its death.

3. Bacteria are admitted by nearly all pathologists to be absent from the blood of healthy persons during life, and yet in from eight hours to four or five days after death, according to the temperature of the air at the time, the previously germless blood of all individuals may be found to be swarming with these organisms in every stage of growth.

4. Whereas blister fluid or serum has been shown to be free from organisms in healthy persons, I have ascertained that, given a febrile patient with a temperature of 102° F., one can determine the presence of bacteria, at will, in any blister-bleb which remains intact for forty-eight hours or more, and this, too, where the patient does not suffer from any specific fever, but merely from pneumonic inflammation. I was led to ascertain this fact by finding, about eighteen months ago, myriads of bacteria in all the blebs of a patient suffering from acute pemphigus, with a temperature of 103°.

5. Lastly, as Dr. Sanderson has shown, a chemical irritant, such as liquor ammoniæ, may be introduced beneath the skin of some of the lower animals in such a way as to "preclude the possibility of external contamination," and yet here, amidst tissues which he has shown to be germless, we may thus within twenty-four hours determine the presence of swarms of germs and organisms in the pathological fluids effused under the influence of the local chemical irritant.

This constitutes, as it appears to me, an exceedingly strong body of evidence tending to show that bacteria are pathological products capable of being engendered within the body after death, or in certain situations during life where tissue elements are dying or where the fluids of the body are notably altered by disease. It is true that the facts and considerations mentioned under 1 and 2 are capable of receiving another interpretation. It may be said, for instance, and it has actually been said by Dr. Beale, that the higher forms of life are, as it were, interpenetrated by the lower forms of life. Speaking of bacteria and their allies, Dr. Beale says:—"I have detected them in the interior of the cells of animals, and in the very centre of cells, with walls so thick and strong that it seems almost impossible that such bodies could have made their way through the surrounding medium." ("Disease Germs," p. 72, 1870.) And elsewhere the same observer says:—"Probably there is not a tissue in which these germs are not; nor is the blood of man free from them." Noting by the way that this latter statement does not accord with the experience of others, I may further mention that some distinguished pathologists, and notably Dr. Sanderson, are also inclined to dwell strongly upon the fact of the wide distribution of bacteria throughout the body—not believing them to be innate or connate (in the mysterious manner imagined by Dr. Beale), but supposing that they have been introduced from without through certain definite channels.

Dr. Sanderson's views on this subject, and the means by which he supports them, are sufficiently remarkable to detain us a few moments. If what he says (*British Medical Journal*, Feb. 13th, 1875) concerning the assumed easy absorption of bacteria from the intestine by lymphatics, and their subsequent passage into the blood, were in correspondence with actual facts, then, in face of the habitual prevalence of such organisms in the intestine, the blood of healthy individuals should scarcely ever be free from them. But this is surely proving too much, since Dr. Sanderson himself assures us that healthy blood is germless.

Again, the other main channel by which, as he says, bacteria may enter into the body abundantly from without is through the bronchi and the lungs. Now, as a result of Dr. Sanderson's oft-quoted experiments in 1871, he claims to have proved "in the most striking manner ..... that *air is entirely free from living microzymes*." Speaking of a previously boiled Pasteur's solution, he says that "no amount of exposure" to air "has any effect in determining the presence of microzymes therein." And yet Dr. Sanderson now talks of the air which is "entirely free from living microzymes" being the channel through which these organisms are introduced into the lungs. It is true that in his recently published lectures this distinguished investigator makes a tacit retraction of his previous statement. He says, in fact, in his first lecture (*loc. cit.*, Jan. 16th, 1875): "It must not be understood that bacteria do not exist in the atmosphere. But their existence there in an active form strictly depends on moisture. They attach themselves *without doubt* to those minute particles which, scarcely visible in ordinary light, appear as motes in the sunbeam or in the beam of an electric lamp. It is by the agency of these particles that they are conveyed from place to place." Elsewhere in the same lecture (p. 70) Dr. Sanderson repeats the statement that "solid materials in suspension in the air" play a principal part in the conveyance of bacteria from place to place, and claims that this was shown by the very experiments of 1871, which then entitled him to express the conclusion that "air is entirely free from living

microzymes." All I can say is, that I have not been able to find in Dr. Sanderson's writings any explanation of this marked change of view, and that I certainly know of no experiments of his which at all establish the fact (extremely difficult as it would be to establish) that bacteria or their germs are conveyed from place to place on the surface of aerial particles, just as his assumed particles of contagion are supposed to be borne about by bacteria themselves. If the theory be true, the conditions for aerial locomotion of contagia are, at all events, getting a little complicated. The contagious particles cannot move about alone; they must engage the services of bacteria to carry them, and these latter porters are unfortunately so delicately constituted that they cannot exist alone in the atmosphere; they can only survive when borne on the backs of some moisture-containing fragments of atmospheric dust, which, though so much heavier than the contagious particles themselves, are freely borne through the air in all directions!

Turning from these statements, therefore, as to the assumed modes by which bacteria habitually gain an entry into the healthy human body, I may say that many of the methods by which Professor Kühne, Dr. Sanderson, and others, have attempted to ascertain whether the different tissues contain actual or "potential" germs are pointless in the face of the statements of heterogenists, since their methods cannot enable them to say, when positive results are obtained, that the "potential" germs from which, as they assume, the organisms have been developed are other than elementary particles of the previously healthy, though now altered, tissues, or that they have not been produced from the fluids which the tissues contain. These experimental observations are not only almost valueless on this account, but they are altogether needlessly complex. Why resort to heated knives, boiled thread, rapid movements, frequent immersions in paraffin at 260° F., paper boxes, warm chambers, &c., when precisely similar results might be obtained by simply leaving the dead animal alone for three or more days, and then subjecting the central tissues of either of the viscera to microscopical examination? So far as the principle of the method is concerned, or the kind of results which it may yield, it makes no difference whether we keep an extracted portion of tissue enveloped in paraffin in a warm chamber for hours or days, or resort to the much simpler method of leaving the animal unopened for several days before submitting its tissues to examination. In either case where organisms are found this fact alone would give us no right to infer that they had developed from pre-existing germs (in the natural history sense of that term); they may, on the contrary, have arisen either by heterogenesis or by archebiosis.

The weight of probability in favour of either of these two possibilities can only be judged of by resort to a different method of procedure. Because in view of the observed absence of bacteria from the tissues of such organs as kidney, liver, or brain, immediately after death, the subsequent multitudinous presence of organisms in these situations would, in the face of satisfactory independent evidence, be more easily accounted for by heterogenesis or archebiosis than by the hypothesis of pre-existing latent or potential germs. By an appeal to evidence of this kind, moreover, we are enabled to test the probability of the hypothesis previously referred to as being supported by Dr. Beale and others—viz., that which assumes the existence of invisible and mysteriously-derived germs of bacteria and fungi throughout the elements of the tissues—an hypothesis somewhat wild in character, which has, I believe, no other foundation than the frequently observed prevalence of organisms in some of these situations.

With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blow-pipe flame; we may boil the fluid, seal the vessel during ebullition, and, keeping it in a warm place, may await the result as I have so often done. After a variable time the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with bacteria and allied organisms, even though the fluids have been so much degraded in quality by exposure to this high temperature, and have thereby, in all probability, been rendered far less prone to engender independent living units than the unheated fluids in the tissues would be. We operate, however, under these disadvantageous conditions in order to make thoroughly sure that, by the preliminary heating, we have destroyed all pre-existing life within the flask; and, notwithstanding such adverse circumstances, we are able to obtain evidence of the occurrence of archebiosis. The researches of Kühne and others have fully shown that the protoplasm entering into the composition of the tissues of warm-blooded animals is coagulated and killed at a temperature of about 111° F.; whilst my own investigations ("Evolution and the Origin of Life," 1874, p. 101) also show that bacteria and allied organisms are killed by exposure in the moist state to a temperature of 140° F.

Hence I contend that the wide distribution of bacteria throughout the human body in connexion with dying tissue-elements in the most varied situations, and also in diseased fluids, is explicable most easily by assigning for many of them an origin by heterogenesis and by archebiosis (though when so produced they multiply rapidly in the ordinary fashion); and that my position—that bacteria are pathological products—is one which may claim to have been fairly established.

On this subject I would only add a word or two concerning the point of view and reasoning employed by those who seem willing to believe in almost any infringement of natural uniformity rather than admit the occurrence of heterogenesis and archebiosis, or either of them alone. The most remarkable recent utterances on this subject are those of Dr. Sanderson, though it is only fair to say that they are somewhat typical of the line of argument adopted by many others.

Whilst admitting that bacteria in their "ordinary state" have been proved to be killed at a temperature of 140° F., and also by immersion in absolute alcohol, Dr. Sanderson assumes that other bacteria germs may exist in an extraordinary state in which they have the power of resisting the influence of this temperature, the influence of absolute alcohol, and even the simultaneous action of both these destructive agents. But if we ask on what amount of evidence this assumption is founded, many may be astonished to find that such an extraordinary belief has been adopted simply because bacteria make their appearance in an organic infusion which has been prepared by macerating an organic extract previously submitted to the influences above mentioned—just as bacteria make their appearance within our closed flasks, whose contents have been previously heated to the higher temperature of 212° F. Has it ever occurred to Dr. Sanderson that another interpretation might have saved him from the necessity of adopting this extraordinary belief?

Again, in his third lecture, the same investigator shows himself for the time similarly oblivious of the point of view of those who believe in archebiosis, whilst the argument made use of to support his own position is of a very surprising nature. After remarking (*British Medical Journal*, March 27th, page 403) that "of all perishable things, protoplasm is amongst the most perishable," he goes on to state that bacteria possess "a wonderful property of passing into a state of persistent inactivity or latent vitality." This is nothing more than an explicit expression of the notion previously referred to, though I wish especially to call attention to the additional "evidence" upon which the view is now based. Dust, containing organic débris, in which, as Dr. Sanderson confesses, he has no proof that anything living is contained, may be added to a fluid at the time barren, though freely capable of supporting life. One of the results of this addition is the appearance, after a short time, of bacteria. A physicist or chemist might conceive it possible that, as a consequence of such admixture, a compound not previously existing might have been more or less slowly formed—as this, at all events, is one of the modes by which new chemical compounds are engendered. But this point of view Dr. Sanderson will not seriously entertain—indeed, his remarks seem only explicable from the point of view of a foregone conclusion that archebiosis is an impossible process, and therefore on no account to be admitted as an interpretation of the facts. In reply to an imaginary objection, alleging that he had no proof that the dust contained anything living, he says with great naïveté:—"True; but I have proof that it contains that which produces life, and express this state of things—viz., the absence of mani-

festations of life on the one hand, and on the other the fact that the stuff in question possesses the power of impregnating something else which before was barren—by saying that the dust possesses latent vitality." The legitimacy of the inference does not seem very apparent to me, if it is to be taken in any other than a poetical sense; yet this is the only evidence adduced in favour of the assumed existence of an extraordinary state in which bacteria may exist—a state in which they are assumed to be capable of resisting influences which are admitted to be destructive to all actually known forms of life. Of course, on the same grounds, the physicist might argue that "friction possesses latent electricity," or the chemist that "oxygen possesses latent acidity," but it seems very questionable whether such statements would be regarded as serviceable additions to science. Neither can we consider that any further light is thrown upon this notion of "latent vitality" by Dr. Sanderson's concluding observations upon the subject, in which he says (*Brit. Med. Journ.*, April 3rd, p. 436):—"The vital activities of the organism are stored up for the future, *the individual being for this very end endowed* with the power of resisting external agencies, and thereby of enduring for an indefinite period." As to such teleological notions I have nothing to say; I prefer keeping to the region of fact and warranted inference. These, however, are the arguments by which a belief in the occurrence of archebiosis and heterogenesis is for the time averted.

Before drawing my remarks on this section of the subject to a close, I would point out that the views admitted by Dr. Beale and those who think with him, those admitted by Dr. Sanderson, Professor Kühne, and Dr. Tielgel, as well as those recognised by myself and others, all coincide with one another on a certain common ground. We are agreed as to the fact that bacteria are abundantly present within the body, or that they may appear therein under certain conditions independent of any immediate external contamination—however much we may differ amongst ourselves as to the interpretation of their presence, actual or possible. Yet this common ground contains an admission which is decidedly inimical to Mr. Lister's theories. Following M. Pasteur, this distinguished surgeon would have us believe that whilst bacteria are disease germs, they do not naturally exist within the body. He has based his antiseptic system of treatment on the assumption that air, or surfaces which have been exposed to it, coming into contact with wounded portions of the body, are the means by which his assumed animated poisons are introduced into the system. But it is, I think, now well known that the whole pyæmic process may be met with occasionally, even where there is no abrasion of the surface of the body. And, moreover, as regards the cause of the disease in persons with open wounds, I may say that Pasteur never seriously attempted to discriminate between the respective effects of the living and the dead elements entering into the composition of atmospheric dust. Effects which were often due to the action of mere organic débris he attributed to the influence of living germs ("Evolution and the Origin of Life," pp. 103—114), and in this respect M. Pasteur has been followed by Professor Lister.

But, as I take it, the essential practical fact which Professor Lister wishes to enforce is that the putrefactive processes apt to take place in wounds ought to be reduced to a minimum, because it seems certain that during such processes poisons are liable to be engendered whose absorption or local influence upon the system may be attended by the most fatal results. Such a notion, which is assuredly thoroughly well founded, may, however, be acted upon by the adoption of the antiseptic system of treatment (or by free exposure of wounds and frequent removal of secretion) quite independently of the question whether mere organic débris may act as ferments, and also quite independently of the further question whether the poisons engendered in wounds are living entities or complex chemical compounds not endowed with the attributes of living matter.

Applicability of the germ theory to artificial tuberculosis, syphilis, typhoid, typhus, relapsing fever, cholera, measles, scarlet fever, small-pox, and other contagious fevers.

I now pass to a consideration of the germ theory in its relation to another class of diseases, although I do not wish to convey the idea that there exists in nature a distinct boundary line, such as my division of the subject might indicate. It must be clearly understood that the local morbid processes or inflammations of a virulent type—which may or may not gradually entail a more general morbid condition—pass insensibly by means of such affections as artificial tuberculosis and syphilis into that class of diseases under which are included such affections as typhus, typhoid, relapsing fever, cholera, measles, scarlet fever, small-pox, and other contagious fevers. Affections like artificial tuberculosis and syphilis might, therefore, have been placed with equal appropriateness in either of the divisions I have adopted.

The treatment of the present part of my subject may be disposed of in a more summary manner than the last, principally because many of the facts and considerations which were advanced in reference to virulent inflammations and their sequelæ, and the presence of independent organisms in the altered fluids and tissues of the body, are also applicable to the question of the relation of such organisms to the more specific contagious fevers.

The case to be made out in favour of the germ theory as applied to these latter fevers is also, in my opinion, much weaker than it is in respect to the virulent inflammations and their sequelæ, since, although such contagious fevers have always been regarded as general and essentially "blood diseases" in only one of those occurring at all commonly in the human subject does it appear that anything like an independent living organism is to be met with in the blood. There is, therefore, here a *primâ facie* inherent weakness in the whole theory, which I think a thorough examination of the question will strongly tend to confirm rather than dissipate.

The reasons relied upon in favour of the germ theory, as applied to these diseases, are of a purely *à priori* or theoretical nature, and such as I have already referred to. They are, in fact, based upon the assumed nature of contagium, and upon its assumed mode of increase within the body. How little conclusive such *à priori* reasons are, and how the facts may be otherwise explained, I have already endeavoured to show, and as the theory in its applicability to these diseases rests upon absolutely no positive evidence that I am aware of, I am compelled to leave a gap here, and pass on to a brief enumeration of the facts and considerations which seem to tell so strongly against the existence of any causal relationship between organic germs and these specific contagious fevers:—

1. The fact that, with two exceptions, no definite germs or organisms are to be met with in the blood of patients suffering from these diseases, during any stage of their progress.

2. The fact that the virus or contagium of some of these diseases, whatever it may be, does not exhibit the properties of living matter.

3. On the other hand, the virus of most of these contagious diseases with which definite experiment has been made, is most potent in the fresh state, whilst its power very distinctly diminishes in intensity as organisms reveal their presence more abundantly therein—facts which would seem to point to the conclusion, or at least are quite consistent with the notion, that the contagious poison may be a chemical compound which gradually becomes destroyed or modified by the successive changes taking place in association with processes of putrefaction.

4. There is the extreme improbability of the supposition that this whole class of diseases should be caused by organisms known only by their effects.

5. The fact of the sudden cessation, periodical visitations, and many of the other phenomena of epidemics, however difficult they may be to explain upon any hypothesis, seem to oppose almost insuperable obstacles to the belief that living organisms are the causes of such epidemics of specific contagious diseases.

It would seem little better than an ill-timed attempt at jesting to postulate the existence of distinct germs for these several specific fevers, and at the same time to endow such imaginary entities with properties different from those of all known germs. To remain always in the germ stage in media favourable for their multiplication would, even if the imaginary germs were visible, be contradictory to all previous experience; but to suppose, in addition, that such hypothetical invisible entities are capable of resisting the in-

fluence of agencies which have been proved to be destructive of all known living matter would seem to be going altogether beyond the bounds of probability. And, if we look at the question from this point of view, we may regard it as a definitely established fact that the virus of cholera, for instance, is not composed of living germs or particles. Messrs. Lewis and Cunningham have shown (Report, &c., into the Nature of the Agent or Agents producing Cholera, pp. 46 and 57, 1874) that the virus is not appreciably impaired in activity when the fluids containing it have been raised for a few minutes to a temperature of 212° F.; and in reference to this subject they say, "We have seen no living object preserve its vitality after exposure in a fluid to a temperature approaching to 212° F., nor have we been able to satisfy ourselves that anyone else has done so."

In only one of the specific fevers commonly met with in the human subject have organisms been found in the blood: this exception is relapsing fever. There is, however, an affection occasionally communicated from cattle (sang de rete, or splenic fever) in which organisms are also met with in the same situation. But the fact of the existence of actual visible organisms in these cases seems altogether robbed of its significance after the occurrence of archebiosis and heterogenesis in diseased fluid and tissues has been demonstrated. The view, indeed, that the organisms found in these affections owe their origin to certain changes prone at times to occur in the fluids of the body is directly supported by some of the most interesting results of Dr. Sanderson's experiments concerning pyæmia. He tells us that in some of the lower animals artificial tuberculosis and pyæmia are often only different effects of the same cause. That is, that some of the same inoculating material may be introduced beneath the skin of two rabbits, and in the one a slow and more chronic set of morbid changes is induced (tuberculosis), whilst in the other more acute and rapidly fatal processes are established (pyæmia). In the former animal no organisms are to be found in the blood, whilst the blood of the latter, according to Dr. Sanderson, is swarming with them. Changes in the character of the morbid process therefore may occasionally favour the presence of organisms. Nay, further, we see the same kind of difference in another way. Pyæmia and septicæmia as they occur in some of the lower animals differ in one very notable respect from the same diseases as they occur in man. Whilst in the lower animals bacteria are to be found in the blood of the living animal, in man they are always absent during life. With such facts as these before us, and others previously referred to concerning the absence and presence of organisms in blister-fluid from different individuals, it need not excite much surprise if we find that organisms are to be found in the blood of persons suffering from one or more of these specific contagious fevers.

There are, however, three other diseases of this class in which organisms, though absent from the blood, are to be met with in those parts of the body which are severally the special seats of morbid change. The three diseases are—vaccinia, ovine small-pox (which seems to be altogether similar to the disease occurring in man), and typhoid fever.

That the organisms of the vaccine vesicle have any significance other than from being possible instances of heterogenesis or archebiosis, I find it difficult to believe. Even if the contagious property of the fluid be resident in some of its particles, as the observations of M. Chauveau and Dr. Sanderson seem to prove, still such particles may exist and yet not be the independent organisms existing in the same fluid. The fact that as the organisms increase in the fluid with age the virus loses its intensity, and the fact that it may remain potent even after prolonged periods of desiccation, are both of them strikingly opposed to the notion that the living organisms of the fluid are its active elements in a specific sense. On the other hand, it does appear, from the experiments of the late Dr. Henry, of Manchester, that vaccine virus loses its intensity when subjected to a temperature of 140° F.

In ovine small-pox we have, as Dr. Klein's very interesting researches have shown (Proceedings of the Royal Society, No. 153, 1874), a local appearance and active growth of organisms taking place in the skin in connexion with its characteristic pustules; whilst in typhoid fever we have also an active growth of rather different organisms in the substance of the ileum, and more especially in the tissues constituting Peyer's patches—that is, in connexion with the anatomical marks of this disease. (*Brit. Med. Jour.*, Dec. 5th, 1874.) But just as a mere chemical irritant (ammonia) injected beneath the skin of a rabbit produces, as Dr. Sanderson tells us, a local inflammation in which the fluids effused swarm with bacteria, why may not the morbid processes taking place within the skin in small-pox engender irritants which may lead to the appearance of somewhat similar products? So that, in face of the evidence already detailed concerning the occurrence of heterogenesis, the presence of organisms in connexion with small-pox lesions may be readily accounted for, without the necessity of attaching any very important *rôle* to them. And as regards the presence of organisms in Peyer's patches and adjacent parts, in cases of typhoid fever, no greater importance could be accorded to this association by any but enthusiastic germ theorists. For, even if the reasons above alluded to were not very influential with them, there is another mode of looking at the matter, from quite an orthodox point of view, which would equally assign to the local development of organisms a very subordinate *rôle*. Morbid tissues are generally admitted to form a favourable nidus for fungoid growths, and the intestine is known to contain the germs or spores of such bodies. The flourishing growth of leptothrix and fungi in the diseased mucous membrane may therefore be only another example of an already well-known class of effects; so that, looking at the question from all sides, it seems to me, in the present state of our knowledge, to be extremely improbable that these newly discovered organisms have any causal relationship to typhoid fever.

It only remains for me now to make a few very brief concluding observations concerning—(1) Pasteur's recent important modifications of his germ theory of fermentation; (2) upon the degree of relationship existing between fermentation and zymosis; and (3) as to the probable mode of action of ferments and contagia.

1. Pasteur has, within the last two years, made a most important modification in his theory of fermentation (*Compt. Rend.*, 1873–74). Whilst he formerly held that fermentations and putrefactions are chemical processes initiated by independent organisms (bacteria and their allies), and taking place in correlation with their growth and multiplication, he has of late shown that similar phenomena may be initiated by the chemical processes taking place in the tissue-elements of certain fruits and vegetable tissues, when these are placed under certain abnormal conditions. Grapes, for instance, suspended in an atmosphere of carbonic acid will undergo fermentation, so as to generate alcohol and other products, even without the presence of torulæ or allied organisms. Other fruits and vegetables treated in the same way behave more or less similarly. Organic multiplication of independent organisms has therefore now been shown by Pasteur himself and his followers, not to be an essential factor in the process of fermentation. With this admission I believe it will be found impossible hereafter consistently to entertain an exclusively "vital" theory of fermentation, and equally impossible to resist accepting the broader physico-chemical theory, and with it the almost inseparable correlative doctrines of archebiosis and heterogenesis.

M. Pasteur, in fact, now proves that fermentation takes place under the influence of altered chemical (nutritive) processes taking place in unhealthy vegetal tissue, just as we know that similar processes may be initiated under the influence of a physico-chemical process brought about by finely divided platinum. As Döbereiner pointed out, this material "has the power—and many organic substances have a similar power—of absorbing oxygen from the air, and bringing it into a condition in which it can unite with other substances with which it would not otherwise enter into combination at low temperatures" (Beginnings of Life, vol. i., p. 409).

And MM. Lechartier and Bellamy, following up the recent experiments of Pasteur, have found (*Compt. Rend.*, Nov. 2nd, 1874), that in these modified processes of fermentation, taking place in vegetal tissues, independent organisms, though they are usually absent at first, not unfrequently make their appearance after a time. In the process as it occurs in beet-root and in the potato, on the other hand, bacteria habitually spring into existence or reveal them-

selves in great abundance soon after the commencement of a well-marked process of fermentation. M. Pasteur will, I suspect, find it difficult consistently to account for these facts without admitting his long-postponed acceptance of doctrines of "spontaneous generation."

2. Respecting the degree of relationship existing between fermentations and zymotic processes, something more definite may now be said. Between the ordinary, previously known forms of fermentation, and zymosis, a most fundamental difference exists which has hitherto been far too much lost sight of. It is this. Whereas, in an ordinary fermenting fluid the changes initiated by a ferment take place in a mere isolated mixture of organic substances, in zymotic processes the changes initiated by contagium occur in the fluids and tissues of a complex living body. That this latter fact does exercise a very important influence, and that the two processes are not so similar as they have been supposed, we may now more readily recognise since the processes of fermentation occurring in vegetal tissues have been investigated. The relationship existing between zymosis and these modified processes of fermentation taking place in fruits and tubers seems, indeed, far more close than that between the zymotic processes in animals and ordinary kinds of fermentation.

In the process occurring in vegetal tissues, as well as in those morbid processes which take place in the animal organism, the presence of rapidly multiplying independent organisms is an occasional rather than a necessary feature. Though usually absent in other allied processes, yet we find organisms invariably manifest themselves throughout the tissues of beet-root and of the potato, when these are placed under certain abnormal ("unhygienic") conditions. And though usually absent from the blood of persons suffering from specific contagious fevers, yet do we also find organisms invariably showing themselves in the blood of persons suffering from some of them—such as relapsing fever and splenic fever. Nay, further, under the influence of a "change of conditions" alone we may initiate these modified fermentative processes in vegetables—that is to say, in ordinary parlance, the processes may originate "spontaneously" or *de novo*. But if the modified activity of tissue-elements suffices to initiate such morbid processes in the vegetal organism, why may it not occasionally do the same in the animal organism? This is a point of view which seems too valuable to be lost sight of, more especially in the face of the results yielded by our flask experiments.

3. In conclusion, I would maintain that the facts already known abundantly suffice to displace the narrow and exclusive vital theory, and to re-establish a broader physico-chemical theory of fermentation.

Whether the "ferment" in any given case be an independent living organism, a tissue element, a fragment of not-living organic matter, or some mere physico-chemical influence (as in the case of the action of finely divided platinum), the initiated fermentative change is in each case a result of chemical action. And similarly with regard to "contagium," whether it be an altered though living tissue element, a fragment of dead organic matter, a chemical compound (or even the more vague influence of a "set of conditions" which may suffice to generate contagium *de novo*), we have in each case to do with gradually initiated chemical changes, distinctive in kind and gradually terminating in one or other of the recognised varieties of zymotic affections. The changes in each case where we happen to have to do with living ferments or living contagia would be due only to an infinitesimal extent to the organic multiplication of such living units, though the decompositions set up by them in their respective fluids may be such as to lead to the formation of a continuous new birth of independent organisms, all of which exhibit most active powers of multiplication. The organisms produced in such cases are, therefore, only to an infinitesimal extent lineal descendants of the original living ferments or contagia under whose influence such fermentative or zymotic processes were originally established. ("Beginnings of Life," vol. ii., p. 361.)

Thus it would appear that the original notion borrowed from the vital theory of fermentation, that all the organisms met with in a fermenting mixture are in the strict sense of the term lineal descendants of those originally introduced as ferments would disappear with the vital theory itself. Yet this has been the notion upon which upholders of the germ theory of disease have always relied so confidently in explanation of the mode of increase of contagium within the body.

Looking, however, at this question from our new point of view, may we not say that chemical changes established in some one tissue, or in many, may, by dint of altered blood and other secondary processes, spread so as to be initiated also in previously sound parts; and that thus throughout the body, or in some special regions of it, living tissue endowed with peculiar and poisonous properties, or complex alkaloidal compounds, may be engendered in enormous quantities, some of which may be thrown off from this or that surface and act after the fashion of "contagia" generally?

CASE OF LARGE MALIGNANT TUMOUR OF THE ARM REQUIRING AMPUTATION AT THE SHOULDER-JOINT.

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I AM indebted to my dresser, Mr. Hatch, for the notes of the case detailed below; and to my former dresser, Mr. Tirard, I owe my thanks for having made the original and beautiful sketch from which the engraving has been taken. Mr. Atkinson, of King's College, was kind enough to make the microscopical examination of the tumour for me.

R. C——, a native of Herefordshire, was admitted into King's College Hospital, under the care of Mr. Smith, on Feb. 6th. Is a fairly nourished man; has a large tumour on the right arm, of a conical shape. It commences about four inches and a half below the coracoid process of the scapula, and extends downwards close on to the elbow-joint,

which, however, is not implicated. The olecranon can be plainly seen and felt; the condyles of the humerus can also be felt with difficulty, but their outline is obscured by the tumour. The arm can be flexed to a right angle, further flexion being prevented by the bulk of the tumour. This flexion has been gradually lessened by the growth of the tumour.

The tumour is fairly symmetrical on both sides, and affects the whole circumference of the arm; there is a depression on its front surface; it is hard and nodular to the feel in some parts, softer in others; the veins over the surface are much enlarged, and the skin shining, tense, and red over parts, but not adherent; the axillary artery can be felt passing over the inner side of the tumour, and the tendon of the biceps is tightly stretched, as if that muscle was pressed forwards; the axillary glands are not affected. On moving the tumour the head of the humerus can be felt to rotate; there is no crackling; no bruit heard. On measuring it longitudinally, in front from four inches and a half below coracoid process to elbow-joint, nine inches; behind, ten inches. Circumference at apex, seventeen inches; about middle, nineteen inches; two or three inches

lower down, eighteen inches. The radius of left arm dislocated backwards, and arm bent at a right angle.

History.—The patient was very strong in health, but no evidence of tumour in family; father and mother both died about eighty years of age. In 1872 he was working in Ireland. Had an attack of jaundice there, for which he went into the hospital, and remained till May of the same year. As he felt the cold and damp of Ireland, he returned to Herefordshire in the following August, when he began to feel pain and weakness in the right arm, and was unable to grasp anything. This continued till August, 1873, the arm gradually becoming weaker and more painful. He then consulted a medical man, who advised him to go to the Hereford Infirmary, which he did, and was treated there for six weeks, but was told that an operation was necessary. He continued to do light work until Christmas, 1874, the tumour increasing all the time, but without much actual pain. He can give no idea of its size when first noticed by the medical man, but it commenced in the middle of the arm, extending all round the limb, and up and down. There was no sense of crackling on pressure; very little pain; changes in the weather seemed to cause what there was, and using the arm.

Operation.—On Feb. 20th the urine was examined—no albumen found; liver rather small; lungs sound; heart also normal. On again measuring the tumour it was at apex 17½ in. round; at middle 19¾. His temperature was 100°. On the same afternoon Mr. Smith operated, the patient being under chloroform. The arm being held by an assistant, he began by cutting the anterior flap from the coracoid process to the apex of the tumour, rounded it off, and then, cutting upwards along the outside of the arm, finished the flap. The arm was then adducted in order to throw out the head of the bone, a procedure rendered somewhat difficult by the fracture of the bone at the apex of the tumour. The joint was then opened, the knife passed behind the head of the bone, and the posterior flap cut, Mr. Bell compressing the subclavian, so that very little blood was lost. The axillary artery with some smaller ones were tied, and the surfaces, wiped with chloride of zinc, were adjusted, and sutures put in. The whole was then dressed with dry lint, a pad of wool placed over it, and a towel held on with strapping.

On examination of the amputated arm the tumour was found to weigh seven pounds, and on making a section of the growth it was found to have involved and destroyed the greater part of the humerus, and the elbow-joint itself was slightly impinged upon by it. The cut surfaces of the growth presented in parts a mottled appearance. It was much harder at the centre than at the circumference. On microscopical examination, the elements were found to consist of spindle-shaped cells, and masses of cells resembling myeloid. These latter existed wholly on the outside of the growth. At the centre of the growth the mass of the tumour consisted of large, irregularly-shaped cells, resembling the cancer cells of the medullary variety, exhibiting distinct nuclei and nucleoli. In many cells two nuclei were visible.

After the operation the patient did well, with the exception of a smart attack of erysipelas, which spread over the shoulder and back, but which, as is usual when the attack subsides, left the wound nearly healed. In seventeen days he was able to sit up, and he was discharged from the hospital in exactly a month from the date of the operation.

So large a tumour as is presented here is seldom met with on the arm nowadays, for it is generally the case some surgical operation is performed before the growth attains to such a size. Fortunately in this case the patient applied just in time, for had a few more weeks elapsed the growth would have involved the parts so close to the joint that amputation of the arm alone would have been impossible. As it was, in order to avoid trenching on diseased structures I was obliged to be very careful and very sparing of my flaps, and I was only just able to make them meet satisfactorily. The case ran the course of most instances of amputation at the shoulder-joint, which, so far as my own observation goes, is by far the most successful of all the great amputations; and even in military practice, as shown by Larrey in the wars of the French Empire, this large mutilation is attended by great success.

Wimpole-street.

STRYCHNIA POISONING.

RECOVERY BY HYDRATE OF CHLORAL.

BY M. CHARTERIS, M.D.,
PHYSICIAN TO THE GLASGOW ROYAL INFIRMARY.

P. G——, a butcher, aged thirty-nine, while in a despondent state, occasioned by the loss of money, went into a chemist's shop in High-street, Glasgow, on March 12th, and bought two sixpenny packets of "Gibson's vermin-killer." Afterwards he adjourned to the private room of a public-house in the same street, and ordered a glass of whisky and a bottle of ginger ale. He emptied the contents of the packets into a tumbler, dissolved them with whisky, and pouring on this some ginger ale, drank the mixture. Observing, on finishing, some grounds remaining in the tumbler, he put in more ginger ale, and left a pretty clean glass. This was about 11.30 A.M. Having done this, he went across the street to the shop of a neighbouring butcher, and, complaining to him of feeling giddy, desired to sit down. Shortly afterwards he had what the people of the shop imagined to be a kind of fit, which lasted a few seconds. On recovering from this he told what he had done, but his statement was not believed. He had a succession of fits, as they were termed, and an emetic of hot water and sulphate of zinc was given at 1 P.M., an hour and a half after taking the poison. Only partial vomiting ensued. The fits becoming worse, he was taken to the Royal Infirmary at 3.30 P.M., and placed in a side room. His condition on admission was as follows:—His body was covered with a clammy sweat, and the expression of his face was suggestive of the most painful and intense anxiety. His pulse was quick and full, from 110 to 120 in the minute. The slightest movement in the room made him start, and the gentlest touch on the hand, arm, or body at once threw him into a violent spasm. His body then became bent, his head and feet forming the extremities of the arch. His eyes were large and prominent, rolling about from side to side, while the pupils were normal in appearance. His nostrils were twitching and dilated, and froth mixed with blood escaped from his mouth. His lower jaw was protruded, and drawn back with a loud cracking sound. His arms were extended, and his hands firmly clenched.

These alarming phenomena, lasting for about half a minute, were followed by an interval of comparative calm, during which the patient could answer questions. The slightest elevation of the tone of the voice or movement of the bed-clothes at once reproduced them. The spasms on admission occurred every ten minutes, and increased in severity and frequency until the specific treatment employed had its effect.

Treatment.—On admission, the house-physician passed, with difficulty, the stomach-pump. As might be supposed, considering the interval that had elapsed from his taking food, nothing was withdrawn but some sour watery fluid, probably the water which had been given to him previous to his admission to the hospital. Shortly afterwards, one drachm of the syrup of hydrate of chloral, strength 10 grs. to the drachm, was given to him. This was at 4.50 P.M. Twenty minutes afterwards the dose was repeated. Very little effect seemed to be produced; and at 5.30 P.M. two drachms were given. Immediately afterwards there was a severe and prolonged spasm, succeeded by a flaccid state of the muscular system, hurried respiration, quickened pulse, and drooping eyelids—phenomena which indicate the effect of chloral. The patient now answered questions readily, expressed great anxiety as to his recovery, and, using a very characteristic simile from his occupation, said the pain he endured all down his back was "just as if he was being boned." I allowed an interval of half an hour to elapse before giving him another drachm of chloral, during which he had four spasms, but their intensity and severity were diminished. After the administration of this dose, though the muscular twitching continued at varying intervals, and could be aroused by touching the skin, yet the acute and prolonged spasms were not evoked. At 7 P.M. another drachm of chloral was taken, and again at 9·30 P.M.

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At 2 A.M. the report states that he had dozed occasionally, but started up at the slightest movement in the room. At 3.15 A.M. the patient was restless, but with no spasms; pulse 118, of good quality; he was perspiring freely. At 9 A.M. he complained of pain over the bladder. He could not pass urine, although anxious to do so. He had not voided urine since 11 A.M. on the previous day. Catheter passed, and ninety ounces of amber-coloured urine taken away.

During the next two days patient complained of pain in his back, as if he had been well whipped. His diet consisted of milk and ice, and in four days he had completely recovered, and said that if he did wish to die he would choose a less painful death than "Gibson's vermin-killer."

Two packets (6d.) of the vermin-killer were analysed. Dissolved in an excess of alcohol, and filtered and evaporated to dryness, the characteristic reactions of strychnia were given with bichromate of potash, peroxide of manganese, nitric acid, &c. It was found that each packet contained fully two grains of strychnia; so that, even allowing that some of the powder did adhere to the sides of the vessel, the man must have swallowed four grains of strychnia. That recovery should have followed such a dose seems almost incredible; and in my opinion, and in that of others who witnessed the case, was entirely due to the action of the hydrate of chloral. It may be mentioned, to account for the slowness of the action of the poison, that the man took it after a most substantial breakfast of ham and eggs &c., and in addition he was a most powerful, healthy man, weighing fully 15 st., and 4 ft. 11 in. in height. In order still more fully to test the strength of the vermin-killer and the efficacy of the hydrate of chloral, the following experiments were made by Dr. Renton and myself, in presence of the students.

1. To a rabbit the powder—a threepenny one—(containing a grain of the poison), dissolved in alcohol, was given by the mouth, or rather attempted to be forced into it. Allowing that probably one-half of it was spilled through the struggling of the animal, death ensued in ten minutes after one spasm.

2. Another threepenny powder was filtered as described, and found to contain fully one grain of the poison. This was again dissolved in alcohol. The solution now measured forty minims. Five minims were injected hypodermically; and immediately afterwards followed by twenty minims of the syrup of hydrate of chloral, and continued as rapidly as the syringe could be filled, until in all one drachm was injected. Though slight twitching was observed, no fatal effect ensued, the animal sank into a tranquil sleep, every muscle flaccid, and ultimately recovered.

3. To show if the quantity injected was really poisonous, five minims were injected into another rabbit. After an interval of eight minutes the ears of the animal commenced to twitch, and two minutes afterwards was followed by a severe and apparently fatal spasm. While thus struggling, as quickly as possible one drachm of the syrup of hydrate of chloral was injected, with the result of the diminution shortly of the spasm, and the recovery of the rabbit.

4. On the following morning both rabbits were apparently well, and could take food. On attempting, however, to move the one on whom the experiment No. 3 was made it was found that it was paralysed from the middle of the spine downwards. It was unable to move its hind legs. Eight minims of the same solution were injected, and death ensued in five minutes. The same injection was sufficient to kill the other one in four minutes.

Remarks.—These experiments amply confirm what was pointed out by Dr. Bennett's committee of the antagonism between strychnia and hydrate of chloral in the lower animals, and it is most satisfactory to know that an otherwise fatal dose of strychnia can be obviated by the administration of chloral in the human subject. That the man would otherwise have died was most evident—that he survived seemed almost miraculous, and was solely due to the action of chloral. This fact should be carefully considered in the treatment of tetanus.

Glasgow.

MR. H. GARROD, Fellow of St. John's College, Cambridge, has been appointed Fullerian Professor of Physiology at the Royal Institution of Great Britain, in the room of Dr. Rutherford, appointed Professor of the Institutes of Medicine at the University of Edinburgh.

Medical Societies.

PATHOLOGICAL SOCIETY OF LONDON.

THE discussion on the Germ Theory of Disease was opened on Tuesday evening, April 6th, by Dr. BASTIAN, whose introductory paper we publish *in extenso* elsewhere. The meeting was a crowded one, and amongst those present were many distinguished epidemiologists and pathologists. Dr. Bastian did not read his paper in full, contenting himself with an abstract, which, however, occupied nearly an hour of the meeting. The chair was taken by Mr. Campbell de Morgan, in the absence of Mr. George Pollock, owing to a domestic affliction. After Dr. Bastian's address, the President, having proposed a vote of thanks to the author in a few well-chosen words, called on Dr. Burdon-Sanderson to reply.

Dr. BURDON-SANDERSON.—When it was suggested to me that I should begin the proceedings this evening, of course I was very reluctant to do so, but as I was told that in all probability there would not be very much time, and as I have not very much to say, I thought there might be an adaptiveness between what I might say and the occasion, and that by consenting to speak first, I at all events should make room for more weighty debaters to come on at the next meeting. The subject which we have before us is said to be the germ theory and the relation of minute organisms to diseased processes. I should like very much, if I could, to limit the subject by eliminating particularly those points on which, perhaps, we may not be so well agreed as we may be on other points. If we are about to discuss the germ theory indeed, I should, as far as I am concerned, decline to enter into the contention altogether. I have listened with very great interest to Dr. Bastian's exposition of his objections to the germ theory. Four years ago—in 1870—I put together what then seemed to me to be the strongest grounds which existed in favour of what I understand to be usually meant by the germ theory—viz., the theory that in those processes of disease in which minute organisms are found the life which interferes in those processes is not only the life of the tissues themselves which are the seat of disease, but another kind of life is introduced. On this account of course I was opposed to the views of Dr. Beale, who also believes that in a great many diseased processes particles have to do with the pathological results, but thinks that the life which they enjoy is a life which is derived from the organisms affected by the diseased process. At that time I felt that the great objection to that theory was this. We know, as regards all kinds of protoplasm, that it is not possible for any bit of protoplasm, much less for bits so minute as one must suppose particles of contagium to be, to be separated from the organs to which they belong without losing their vitality; and that seems to me so complete an objection to the notion that the life of such particles (supposing them to have any life at all) is derived from the organism which is the subject of disease, that it is not even possible to think of this theory, much less to accept it. I then developed, to the best of my ability, the reasons which seemed to me to be most weighty in support of the general notion, but I felt at the time that this was a matter of comparatively little consequence as compared with the pathological facts. I brought forward the theory merely as an introduction to certain pathological facts to which I wanted to proceed. Of course I was aware at that time that the views I then brought forward were such as had been brought forward before, and might have been brought forward thirty years before; and in fact I found in the course of a few years that the very arguments which I had used were those which had been used by Professor Henle, the veteran pathologist, as long ago as 1840, in a little book which I got from Dr. Sharpey the other day, called Pathological Researches, by Professor Henle. In that book I found the germ theory developed most completely. I mention this merely to illustrate what I am going to say, that if I had begun in 1840 to discuss the germ theory we might have gone on in this

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Society, supposing the Society had then existed, to have discussed it till the present moment, and we certainly should not have gone any further. We see an example of this sort of thing in the debates in the French Academy. I mention this for the purpose of expressing a hope that we, the members of the Pathological Society, will not emulate the French Academy—that we will not begin a career of discussing theories. If we did so, I think we should be behind the time; for the difference between the present time, 1875, and 1840 consists as much as anything in this, that now we profess at all events to have put aside theories and general speculations of all kinds, and confine ourselves to the observation of facts. But above all we should be proving false to our own initiative, for this Society began as a Society for observation. The purposes and intentions of the founders were to collect facts relating to pathological anatomy, facts which then had been too little investigated. The results of those observations, as we all know, have been built up into that splendid monument of them, the Transactions of the Pathological Society. Well, we have now left this subject, or seem inclined to leave it—I mean the bare recording of anatomico-pathological facts,—and are turning our attention to the investigation of diseased processes, such as the process of cancer, the process of pyæmia, tuberculosis, and now we are beginning with the process of infection. But I do not think that there is any reason why we should, now that we have changed our ground, also change our method. I think that we should still keep to the investigation of facts, clinical facts and pathological facts, as our principal object. Now, with reference to this germ theory, I am quite sure that if any of us were to go and ask any of the eminent pathologists of the present day, especially any of those who have actually been engaged in making those investigations with reference to the pathological products which have called so much attention to the subject of organisms in connexion with diseased processes, they would all give the same answer. Supposing any one of us went to Recklinghausen, Virchow, Chauveau, Paget, and asked the question, "Do you believe in the germ theory?" the answer he would certainly get would be, "I really cannot give you any opinion upon the subject. A great number of observations have been made upon the subject; you must read those observations; then, if you wish to pursue it, you must make observations yourselves, and perhaps at a future time it may be possible to come to a conclusion upon the subject." But if they were pressed to give an answer to the question, "Do you believe in the germ theory?" I believe all of those eminent men would shrug their shoulders. What I have said this for is simply to express a hope that we also, in view of mere theories on the subject, such as the question whether or not these organisms are the cause or the consequence of the diseased processes, or whether they originate from pre-existing germs, or whether they originate in the tissues spontaneously (questions which are utterly beside our pathological mark), can afford to put them by for the present, because they are questions which are not yet decided, and which rest for their decision on observations which we as pathologists have not very much to do with. But after we have made up our minds (if we can only do so) to put these theoretical questions into the cupboard, and turn the key upon them, is there anything else that we can discuss? There are certainly some things, there are a few facts which still remain, relating to the question which may possibly form the basis of useful conclusions. Although the facts are few, they are not so few that it is possible to enumerate them in ten minutes or a quarter of an hour, but I wish to draw the attention of the Society to the few facts which exist which seem really to have a pathological bearing on the subject, because I apprehend that whatever we might think about the germ theory, we all agree that the questions which we have to discuss here are pathological questions, and not questions of biology or natural history. Well, Sir, the first facts that I mention may be called a series of negative facts. Some of these have been mentioned already by my colleague, Professor Bastian. He has mentioned a fact which is a very important one, that bacteria, which seem on microscopical examination to be identical in their character with those which present themselves in morbid tissues, can be introduced into the circulation without producing any pathological results. This is a fact which I stated in this room two or three years ago, and which since that time has been very carefully investigated, and is now generally admitted. This, of course, does not imply even that the pathological organisms which do exist in pathological products are not active, but that organisms which you cannot distinguish from them may exist in other liquids, and that these may be introduced into the circulation without producing marked results. Then we have a series of facts which relate to the production of fever, which are also instructive, and which I can detail to the Society in a very few moments. We know that there is no substance which can produce fever—that is to say, there is no substance which can produce that physiological fact that we call fever, because it presents a series of changes which constitute fever—namely, first a period of latency, then a shivering, then a rise of temperature accompanied with shivering, going on afterwards and then declining; this we call a paroxysm of fever, and this peculiar physiological fact can only be produced by albuminous liquids which are in a stage of septic decomposition. Well, with reference to that fact, of course we know that such liquids also contain bacteria, and we know experimentally that in this case, although we cannot say that the bacteria have nothing to do in the production of the pyrogenic stuff, yet we know that the pyrogenic stuff can be separated from the bacteria, that you can kill the bacteria by boiling them in alcohol, and in this way obtain an alcoholic extract, which still presents a peculiar physiological action—that of producing fever. Well, this a very important negative fact to bear in mind on the threshold. Then we come to positive facts, and these are of two kinds. There are positive facts which relate to inflammation, facts which I developed here, so far as they were then known, about two or three years ago—the fact that in all destructive inflammations, all inflammations which are attended with a destruction of tissue, bacteria are certainly present in the liquids of the destructively inflamed part. When I say destructive, I mean in the strict sense in which I have defined it—namely, inflammations which really do lead to a destruction of tissue. In all such inflammations we are tolerably certain that organisms are sure to be formed. This observation is not an observation made by one man, it is an observation made by observers in different parts of Europe, who work entirely independently of each other. We have an illustration of this independence in the different names which have been given to these things. When I worked at them before, I contented myself with calling them spheroids; others have called them micrococci; others microspora septica; while Billroth has given them the much longer name of cocco-bacteria septica. All these are the same things, they are found under the same circumstances, so that this observation may be considered as an observation about which there can be no doubt. Then of course there connects itself with that the other fact, that all those forms of inflammation which are destructive have in them an infective character—that is to say, they are also spreading inflammation. So that we have along with the first fact the other, that with any spreading inflammation you have always bodies of the character which I have just mentioned existing in the affected tissues. Now, as regards the particular diseases which illustrate this, we can go a great deal further now than it was possible to go at the time I last addressed the Society on the subject. Since that time several important researches have been made bearing on the subject. For example, in diphtheria there have been observations; then there are the very important researches of Heiberg relating to puerperal fever, and several others; but I think the most important of all are the recent researches of Recklinghausen of Strasburg, relating to the process of erysipelas. These observations show particularly a fact which has a considerable importance pathologically—viz., that in spreading erysipelas bacteria are found (I use the word "bacteria" as signifying these round bodies which are properly called micrococci); that these micrococci are found in the lymphatic spaces on the affected skin and in the lymphatics belonging to them, not in the advanced stage of the process, but at the beginning—that is, they characterise what may be called the progressing zone, the edge of extending inflammation in the process, and if an examination of the same skin is made at a distance from the margin of extension no bacteria are found. This not only serves to illustrate the intimate relation of the organisms with the process, but

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serves to show that we have here nothing to do with septic results; we have to do with a phenomenon which is connected with the beginning of the process, and which cannot be considered as a result of the destructive changes to which the process has already given rise. The other set of facts are those to which Dr. Bastian has not devoted very much attention in his address—namely, those which relate to specific diseases. And here I must say that I think his position, and the position of all who adopt the same views, becomes an extremely difficult one. There are two diseases to which one may direct attention, both of which he has mentioned in his address, and I will not occupy more time of the Society than is sufficient to mention them—namely, relapsing fever and the sheep-pox. As regards relapsing fever, everybody knows what the history of the discovery of the peculiar spirilla which exist in the blood of relapsing fever is: everyone knows that they were discovered by Obermeier in the course of the epidemic of relapsing fever that occurred in Berlin a few years ago, that he observed them successively in a number of cases, and that he established the fact that spirilla existed in the blood only during the febrile paroxysm, and that they disappeared at the end of the paroxysm. This fact alone would be a very extraordinary fact on any possible supposition of spontaneous generation. It is impossible to suppose that the moment a patient became febrile these organisms were spontaneously generated, and that the moment he ceased to be febrile they were spontaneously annihilated; and, of course, if you adopt the one conclusion, you must admit the other. Then, again, as regards the formation of these organisms, they are utterly different from any organisms which occur in any septic product; they are altogether specific in their form, perfectly characteristic, and entirely different from anything which one meets with in the tissues of either living or dead animals. If the whole thing had depended upon one observation of Obermeier, one might have said, "Obermeier was an enthusiast; he made observations not reliable; he was convinced that these things existed, and, as microscopical people do, he saw what he wanted to see." But that is absolutely impossible. The observation now has been not only confirmed by the first botanists of Germany, but it has been confirmed by clinical observers in an immense number of cases: for example, in the late report of the epidemic at Breslau, you have a hundred observations in a succession of cases of remittent fever, in all of which, without exception, organisms were found. It has also been confirmed by all clinical observers who have had the opportunity of watching epidemics of relapsing fever since. In the presence of these facts, I confess I must say it seems to me absolutely impossible to suppose that these organisms have the origin which Dr. Bastian attributes to them. As regards the other case, that of sheep-pox, what we have before us is something different. We have two processes going on side by side at the same time. On the one hand, we are able to observe the development of the pustule from its papule to its ultimate pustular stage in the skin of the sheep with all completeness. Having to do with an animal, it is possible, of course, to make observations on the skin at various times during the progress of the disease; consequently we have all the anatomical changes before us, one after another, in a complete series; and, on the other hand, we have a complete series of developmental changes—changes which consist in the development of organisms, and we find that these organisms, beginning in a certain form, and beginning in a certain anatomical part of the skin, always in the superficial layer of the corium, gradually extend from that part, and find their way into the rete Malpighii; that these have a definite anatomical relation to the tissues; that their growth takes place, not in the tissues, but in the lymphatics; and that eventually, as vesicles become formed, you have an orderly series of morphological changes taking place in the liquid contents of these vesicles. So that you have two processes side by side, each of which has a development of its own; on the one hand a development of the pustule, and on the other a development of the organism. There again it seems to me absolutely impossible to admit anything else than that the two processes have some relation to each other. As I have said before, I do not think it at all worth while to mention that the organism is the cause of the process, or that the process is the cause of the organism, for in nature we are accustomed to view phenomena in that way. We are accustomed to the notion, with reference to organic beings, that the environment of an organised being has a constant influence upon it, but that this influence does not make it impossible to suppose that the organism has an influence upon its environment. So that in this case it is not necessary to form any theory as to which is the cause of the other. All that we have clearly to understand is that the two processes are associated together, that both of them are characteristics of the disease, and then leave it to a later period to determine which is to be regarded as the cause, if it must be determined some time or other, and which is the effect. The point we have clearly to satisfy ourselves upon—and I think even the few facts which are already recorded are sufficient for this—is that the organic forms have an inseparable connexion with the diseased process. These are a few facts which appear to me to have a bearing upon the question. Of course the whole question is—Is there a relation between them and the pathological process? In the presence of these facts, I do not think it desirable that we should put aside theories; and I have no objection to anybody putting aside mine, forgetting it, and locking it up; but I do think it is irrational to pass by facts which are established by good observers, which have been confirmed by the most competent persons both in England and Germany, and about which there is no reasonable doubt. If they are true, let us accept them; if they are doubtful, let us wait till better observations have been made; and if they are obviously false, of course let us reject them.

The debate was adjourned to the next meeting.

MEDICAL SOCIETY OF LONDON.

AT a well-attended meeting on March 22nd, Dr. Routh, President, in the chair, Mr. GANT exhibited a man on whom he had performed Excision of the Tongue for Epithelial Cancer.—Mr. DURHAM gave the fellows of the Society a laryngoscopic demonstration of the patient. He also made a few remarks, in which he advocated the use of the galvanic cautery écraseur.

Dr. THEODORE WILLIAMS exhibited a case of Arrested Phthisis, which he had first seen in January, 1871. The chief points of interest in the case were the following:—(1) the rapid rate of contraction; (2) the absence of shrinking of the chest-wall, or displacement of the adjacent organs; (3) the excavation process, which proceeded rapidly, and seemed likely to involve the whole lung. He concluded by remarking that the contraction and obliteration of so large a cavity was, as far as he knew, unprecedented in the records of medical science.

Dr. BROADBENT then read the notes of four cases in illustration of different forms of Uræmic Poisoning. 1. In the first case—a woman aged thirty-one—pain was felt in the epigastrium, legs, side, and back; the urine was pale and deposited much yellow pus, specific gravity 1010, alkaline but not ammoniacal. Three days afterwards the breath was noticed to have an ammoniacal odour, and there was incipient nasal stertor; death took place two days subsequently. The kidneys were found to be in a most advanced stage of tubercular disease. 2. The second case was that of a girl aged eighteen, who, on admission, presented the appearance of a patient in a late stage of typhoid fever. Specific gravity of urine 1003, and great constipation of the bowels. Five days afterwards convulsions came on, followed by coma and death. 3. The patient was a woman aged forty, who, when she came under observation, complained of great pain in the arms and legs, there being a uræmic but not ammoniacal odour of the breath. Specific gravity of urine 1010, with an acid reaction and much albumen; death occurring in five days. The disease was tuberculous, one kidney being entirely destroyed. 4. The fourth case occurred in a woman aged fifty, who, some seven months before admission, had been seized with a maniacal epileptiform attack, followed by almost weekly convulsive seizures. The urine was found to be albuminous, and death supervened in twenty-two days. The kidneys were discovered to be in the contracted granular condition, but not to an extreme degree. Dr. Broadbent concluded by saying that death commonly occurred in uræmic poisoning without previous symptoms of sufficient importance to attract notice.

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THE LANCET.

LONDON: SATURDAY, APRIL 10, 1875.

THE debate at the Pathological Society on the Germ Theory of Disease, which was so ably opened by Dr. BASTIAN in the paper which we publish in full this week, is likely to attract much attention both here and abroad. For some years past the subject has been growing in interest and importance in its bearings on our views of diseased processes, and especially of fevers; and whilst until lately the term "germ theory" has been mainly significant in common opinion of the view held by some on the subject of septicæmia and pyæmia, recent researches have combined to give it a wider and less definite meaning. And although the subject has been treated as a whole by some writers, we believe we are correct in stating that the Pathological is the first Society which has ventured upon a discussion on the relation of bacteria and allied organisms to contagious diseases and fevers. We may remark at the outset, that the two subjects thus grouped together are in all probability essentially distinct, and we almost question the wisdom of thus joining them without, at present, a sufficient knowledge of what they have in common. It must be remembered that the ordinary processes of septicæmia and pyæmia are separated by a very distinct boundary line from the specific contagious or infectious fevers: for, on the one hand, it is a commonly recognised fact that the former may be set up by the contact of any animal matter at a certain stage of putrefaction, and that the greatest variety in the results of the inoculation is observed; whilst, on the other hand, the so-called specific fevers appear to propagate their own species, and to be more definite in their course and the lesions they induce, and the evidence of their originating *de novo* is at present imperfect. Hence we cannot but think that it is a somewhat hasty step to join the two together.

In opening the discussion, Dr. BASTIAN could not at any rate complain that the field was unduly restricted; indeed, the difficulty under which he obviously laboured was to condense his arguments so as to give anything like a fair view of the subject. Nor was the difficulty diminished by the fact that he found it necessary to give his views in abstract only at the meeting—a course which, perhaps, gave a somewhat too dogmatic tone to his opening address, and rendered it difficult to follow his argument clearly. It would be obviously out of place at present to discuss the arguments employed by Dr. BASTIAN in support of his views, and thus to anticipate the discussion, and equally so to enter into any detailed criticism of Dr. SANDERSON'S reply. The strongly negative position taken by Dr. BASTIAN reminds us of the lawyer who "denied everything and insisted on proof"; and it is not a little significant that the opening address should have been so essentially destructive in its character. For Dr. BASTIAN the germ theory is but a theory founded on a false argument from analogy between the processes of contagious diseases and of fermentation—an analogy resting on insufficient grounds even if the fermentation theory of PASTEUR were true—a theory which Dr. BASTIAN rejects in favour of the views of LIEBIG. To Dr. BASTIAN germs, as commonly understood, do not exist, or if they exist they are non-essential. And it is unnecessary to follow him in his arguments as to the mode of origin of bacteria if they possess so little importance as he ascribes to them. So far as bacteria are concerned, Dr. BASTIAN certainly succeeded in summing up very ably the facts which are opposed to the view of their pathogenetic influence, and it will be difficult to contradict them by way of argument only. Many of his arguments were weighty, and his attitude of incredulity, or rather unconvincedness, may perhaps be the wisest in the present state of our knowledge—that is, if it lead to a careful investigation and repetition of experiments, with a view to eliminating all possible sources of error. Yet we cannot but agree with Dr. SANDERSON that the facts already on record as to the *rôle* of other organisms in specific diseases are too numerous and important to be put aside or answered by a preconceived theory, and he did wisely in contenting himself with marshaling some of the chief of the recent researches, without committing himself to any theory upon them. Dr. SANDERSON has so lately given to the scientific world an able exposition of the present state of the subject in his lectures at Owens College that we are not surprised that he contented himself with only a brief summary of it in his speech. It was obvious, too, that he felt that the subject was hardly ripe for profitable discussion, and in this we are inclined to agree with him. Yet it is possible that, if some definite points be kept in view in the discussion, some good may result from it. As we have pointed out, two essentially distinct subjects are involved, having probably only a superficial resemblance. With regard to bacteria, we require to know their relation to blood-poisoning—whether it is accidential or essential—whether, in fact, their presence constitutes and their absence destroys the infective or septic character of the fluid. And, if they be essential, are they epiphytes, such as LISTER considers them; or heterogenetic by a process of endogenous formation or degeneration in the protoplasm of cells, as Dr. BASTIAN seems to maintain; or do they, as Dr. BEALE considers, originate from latent germs interpenetrating the tissues, but which only develop under certain conditions? And if they are essential and constant in infection, are they of one kind, or do different varieties exist under the same external form, as Dr. SANDERSON suggested? And do the varieties in their growth in different fluids depend on the fluids or on a difference in their essential character? These are questions which can only be answered by careful experiment, and, when and however answered, we do not see that the answer will necessarily affect the question of the relation of the lower organisms to contagious specific fevers. If the discussion should be directed to the consideration of facts bearing on these questions, with a view to the explanation of discrepancies and the detection of fallacies in the methods or results of observations and the suggestion of new ones, it will not be without value; but if merely rival theories based on hypotheses are contested, the debate, however in-

teresting, will be as barren in results as those in the French Academies which Dr. SANDERSON so justly ridiculed.

THE Bill prepared and brought before Parliament by Colonel BERESFORD, Sir CHARLES RUSSELL, Mr. FORSYTH, and Mr. RITCHIE, "for making more effectual provision for a constant water-supply and for the protection of life and property against fire in the metropolis," has been printed and circulated, and we are enabled to lay before our readers a sketch of the proposed measure, to which we alluded briefly last week. The Act is to be known as the "Metropolis Water Act, 1875," and the preamble is as follows: "Whereas the vesting in one competent authority of the several undertakings of the eight Companies now supplying water in the metropolis and its neighbourhood would be of great public advantage, as tending to secure a constant supply of water to all classes of houses, and an efficient pressure for the extinction of fires and other purposes, and as tending to economy and the consequent provision of funds by means whereof the poorer class of houses may be supplied with fittings for receiving a constant supply; and whereas larger powers and greater freedom of action may be properly entrusted and allowed to a Public Commission than to Companies carrying on undertakings for their own benefit ..... be it enacted," &c.

In short, the whole object of the proposed Bill is to substitute a "Metropolitan Water Commission" for the eight existing Companies. The Commission is to be independent of the Metropolitan Board of Works and the Commissioners of Sewers, and is to be composed of persons nominated by Her Majesty. It is to have the power of acquiring and disposing, as it may think fit, of the undertakings of the various Companies, the shareholders and officers of which Companies are to be compensated by life annuities calculated on an equitable basis. The property acquired, the Commission is to have power to do the following things—viz.: "To combine, as far as physical conditions admit, the sources of supply and works of the several Companies; to dispose of works, lands, and property becoming superfluous in consequence of such combination; to continue any supply given by a Company at the time of the transfer, and to extend from time to time this supply within the parishes supplied; to put the works into a condition for maintenance of constant supply at high pressure; to provide and place hydrants and apparatus for high-pressure supply in case of fire, and for other purposes; to provide and fix apparatus for cleansing and watering streets, and for other sanitary purposes."

The water-supply of London is admitted on all hands to be inferior in quality and insufficient in amount, and, since any adequate improvement is impossible so long as responsibility is divided as at present, it is obvious that the change contemplated in Colonel BERESFORD'S Bill is one of vital importance, and one without which no reformation can be looked for. A Water Commission once established, there would be little difficulty in enlarging its powers, if necessary; and since it would be a public body, and one subject to parliamentary control, it could not turn a deaf ear to public opinion; and although the word "health," strangely enough, does not occur in the preamble, there can be no doubt that the Commissioners would recognise the close connexion between the public health and the supply of water. The maintenance of a constant supply is certainly a matter of prime importance, as thereby would be rendered unnecessary the filthy and ill-kept receptacles which are honoured by the name of "water-cisterns" in some of the dwellings of the poor. The abolition of cisterns, however, has this disadvantage—that, in the event of injury to one of the mains, the entire district supplied by that main is wholly deprived of water for the time being—a matter which might be fraught with very serious consequences during the summer months. The various alterations and improvements contemplated in the Bill are doubtless of very great importance, but they are all secondary to the *source* of our water-supply, which, for a great part of London, be it remembered, is that gigantic sewer which we call the River Thames. The source of supply is not mentioned in the Bill, and the alteration of the source of supply is not included among the powers given to the Commission. This important power, however, would certainly follow; and although the extinction of fire seems to take precedence of the extinction of zymotic diseases, there cannot be a doubt that this latter duty would be forced upon the Commission, and that the Chief Commissioner would find his post a warm one did the body under his control continue to serve out to the inhabitants of London "living organisms and organic matter," as too frequently is the case at present.

The Times, in a leading article on this subject on Monday last, raised the question as to the feasibility of a "double supply": one for drinking and cooking; and another for cleansing, street-watering, sewer-flushing, and the extinction of fire. This, indeed, seems a point well worthy of consideration; and when the operations of the Water Companies are amalgamated, such a course would be possible, provided the necessary funds were forthcoming. The water-supply of London has been estimated at fifty gallons a head. Of this scarcely more than one gallon a head is used for drinking and cooking; so that of the 200,000,000 gallons, which is the estimated daily supply of the metropolis, only 4,000,000 gallons need be absolutely pure. This could easily be obtained, on the north side of the Thames, from the Hertfordshire springs which supply the New River Company; and on the south side the water of the Kent Company, which is acknowledged to be the purest which comes into London, might be employed solely for drinking purposes. It has further been pointed out that there would be no absolute necessity to carry the pipes for drinking-water to every floor of every house, since the labour of transporting the very small quantity required would not, under any circumstances, be excessive. Drinking-water being provided for, the necessity of elaborate filtration of the rest of the supply would not exist, and in all probability the supply would in consequence be more abundant. It is certain that in dry weather our sewers are very inadequately flushed; and during the past few weeks, which have been remarkable for an almost utter absence of rain, the walker in the London streets must have become aware of an ammoniacal odour issuing from the gratings of all our sewers. It is always in dry weather, and especially when

there are high winds and the atmosphere is impregnated with dry pulverised particles of road dirt, that complaints of bad smells are heard. A thorough flushing of the sewers and watering of the streets would completely obviate this inconvenience. Two or three years ago a Company (since defunct) was started for conveying sea-water to London by means of an aqueduct from Brighton. By using sea-water for the purposes last alluded to we should not be open to the charge of robbing another district of the water which of a right belongs to it, and it is possible that sea-water would prove a more effectual moistener of our roadways than fresh water. We surely do not make all the use we might of that element of which we are traditionally supposed to have complete command.

JUDGING from the reports in the local newspapers, the first annual meeting of the Birmingham Medical Institute, to which we briefly alluded last week, was by no means lacking in interest or excitement. The practical business of the meeting was to determine whether the original Committee had acted prudently and in accordance with the general wishes of the profession in admitting professed homœopaths as members of the Institute. We need not now enter into the details of this contest, as the various phases of the question have from week to week been presented to our readers by the champions of one side or the other. The result of the meeting was, as we have already announced, that 64 voted for the adoption of the report of the Committee, thereby acquiescing in the decision of the Committee to admit homœopaths, while 35 voted against it. But, notwithstanding the laboured pleasantries of Mr. ALFRED BAKER and the strong personalities of Dr. HESLOP, this vote cannot be fairly regarded as a genuine victory for the friends of homœopathy, or as expressive of the opinion of the great bulk of the profession. Indeed, a reference to Mr. PEMBERTON'S letter (p. 524) will show that it is not so; for of the 460 replies 436 stated that, in their opinion, professed homœopaths should not be admitted as members of the Institute, while only 19 were in favour of their being admitted, and 5 were neutral. This majority, large as it is, is greatly increased if we add to the 436 the remaining 540 who, desiring that things should remain on their old footing, did not deem it necessary to send replies. It follows therefore that 976 members of the profession out of 1000 were opposed to the admission of professed homœopaths as members of the Medical Institute. Here we have at the same time almost unanimous approval of the course of action adopted by Mr. PEMBERTON and a sharp reproof to Dr. HESLOP, who has, in addition, had to suffer the humiliation of confessing that when he wrote the letter against homœopathy, extracts from which, furnished by a correspondent, are published in another column, he was entirely ignorant of the subject, and that, notwithstanding the violence of his invectives, he echoed merely the prejudices of his teachers.

Earnestly and sincerely do we protest against the action of the Committee of the Institute, and regret that so many estimable and well-meaning members of the profession should have been so misled by false notions of toleration as to approve the conduct of the oligarchy that arrogated to itself the right to interfere and tamper with the canons of professional ethics. If the admission of professed homœopaths into the Institute had been merely a question of liberality and toleration of opinion, we should have been glad to encourage and support it, but from the first it has appeared to us in a very different light. It has not been with us a question whether homœopathy is a better system than rational medicine or not, or whether the differences between the two are sufficiently important to be the subject of contention or not, but whether we should admit into professional associations persons that assume a name intended to imply superior skill and learning in those that bear it, and ignorance and stupidity in those that do not. If a man conscientiously profess and practise homœopathy he should be the object of commiseration; but if he profess homœopathy without adhering to its practice he forfeits all claim to confidence and respect. Unless a homœopath always and exclusively practise homœopathy, he is not justified in maintaining a name that is "calculated to mark himself from the general body of the profession." As well might he call himself a hydropath because he sometimes employs baths in the treatment of his patients. If a practitioner of medicine avails himself of all the resources of therapeutics, he thereby ceases to be a homœopath, even though he does sometimes employ remedies apparently homœopathic in their action. But if, in spite of this, he still retains the name, he must not be surprised if others should accredit him with all the follies of Hahnemannism, or should allege that he makes use of a name intended to decoy the public into the belief that his mode of practice is special or peculiar. Looked at from this point of view there is no possible ground for toleration, and what passes by this name is either indifference or ignorance. How ignorance may prevail has been strikingly shown in the recent dispute at Birmingham. All the local newspapers have made the confession that they know nothing of Hahnemannism or homœopathy, or of the matters in dispute. Yet their cry has been—"Toleration! toleration!" and by officious interference they have actually forced the homœopaths into the Institute. We say it with all deference to the non-medical press, but we maintain that the question of admitting homœopaths into a medical institute is not one to be discussed by those, whether in the profession or out of it, who confess that they are ignorant of the essential principles and doctrines of homœopathy.

Annotations.

"Ne quid nimis."

MR. CATHORNE HARDY AND THE MEDICAL OFFICERS.

THE Minister for War, when pressed the other evening in the House to make some statement as to his intentions in regard to the Medical Service, declared that he had given the subject great consideration, but that it was one of much difficulty, owing to the different views entertained by the medical officers themselves in regard to their grievances. We have always understood that while different opinions existed as to what was the best system of organisation, medical officers were nevertheless agreed as to what they had to complain about, and the nature of their require-

ments. They complain that the terms of the Royal Warrant of 1858 have been altered or set aside by subsequent documents, some of which were of inferior authority, and that the privileges and allowances of their relative rank, which were guaranteed in 1858, have since that time been curtailed in several respects without any compensatory advantage to themselves; that, whatever might have been the merits or demerits of the new system, the late Government, in a harsh and peremptory fashion, deprived all incumbents of their regimental appointments, and imposed new and increased duties on medical officers, while, practically speaking, they at the same time diminished their incomes. Moreover, the existing Warrant is regarded, in general terms, as an emasculated kind of document, and where it differs from its predecessor of 1858, medical officers allege that the difference is adverse to their interests. If Mr. Gathorne Hardy has read some of the various pamphlets by which he has been flooded, he must have gathered thus much in outline, and a very little leisure will enable him to refer to the same sources of information in order to fill up the details.

Next as to the remedies. For these we do not think he could do better than follow the direction indicated by the new Naval Medical Warrant. Fifteen years' service in a subordinate rank should be the *maximum* length of service in that rank. We put it to Mr. Hardy, as a man of business, whether he expects that a duly-qualified medical man will first of all compete on admission, then go through a course at Netley, again compete before getting a commission, and after having undergone a special supplementary training to make him a medical officer, an analyst, sanitarian, and what not, be content, with the present pay and prospects, to serve on for fifteen years, during nearly the whole of which time he may reasonably expect to be expatriated? The pay is certainly insufficient to tempt men of the best stamp to do so, and as, practically speaking, a medical officer has no relative rank, he is not influenced by the same considerations that affect the military officer in that respect. There must, then, be hope of some kind for him, and hope of promotion is the best and most legitimate kind of hope. So long, however, as matters remain as they are, where is this encouragement to come from? And we appeal again to Mr. Gathorne Hardy's common sense to say whether, if zeal is to be maintained, promotion by selection should not be more frequently had recourse to than it is at present. There may be other things, but here are sign-posts indicating the main road which no politician can well miss seeing.

THE "PRACTITIONER" AND SECRET REMEDIES.

IT is not our practice to criticise reviews which appear in our contemporaries, and nothing but a sense of duty would make us do so. *Quis custodiet ipsos custodes?* is a question more easily put than answered, and as a general rule the sense of public duty must be the chief guide. But there are cases where silence is harmful, and we must make an exception to our general rule in the case of a review in the April number of the *Practitioner*. The *Practitioner* has been so well known as the mental progeny of the late Dr. Anstie, to whose exertions, indeed, it owes not only its origin, but its present reputation, and his name was so long and honourably connected with this journal, that we cannot but feel a peculiar interest in the matter. If there was one respect in which our lamented colleague was pre-eminently distinguished, it was in his high sense of professional honour, and we cannot for a moment suppose that he would have tolerated any deviation of the kind in his journal. Yet we find in the number in question a review, extending over nearly four pages, devoted to the exposition of a recent work which treats mainly of secret remedies. Of the book in question we, in common with most of our contemporaries, have taken no notice, preferring to pass it by without giving it the publicity which the condemnation it deserves would entail. It professes to propound a method of almost certain cure for phthisis; and is filled with abuse of all who venture to differ from the author, and the promise of nearly certain cure to all who seek his advice. And whilst we find it stated that one part of the method affords a specific remedy for the tubercular diathesis, we are told elsewhere that another part will effect an almost certain cure in a far more rapid manner, and even when the first is hopeless. Yet when we come to inquire what is this *elixir vitæ*, we are treated to a series of tirades against the profession, and a lamentation over the fate of inventors in general, and are calmly informed that the author does not intend to divulge the system "except to those whom it may personally concern." Were this a review, we need hardly say that it would be our duty to expose more fully the assumptions and statements, as well as to point out the fallacies in the alleged proofs of the efficacy of the methods; but enough has been said to show the character of the book. And of this work we find a review which is not only not condemnatory, and does not even "damn with faint praise," but is actually laudatory of the author and his theories, and the reviewer meets the most objectionable portion of the work with the wish "that the information were more definite and concise," whereas no information whatever on the point is afforded.

We cannot for a moment suppose that the Editor of the *Practitioner* is at all directly responsible for the review; we have little doubt that he entrusted the work to someone else, without exercising that supervision over him which was needed; but it is greatly to be regretted that he should have allowed the review to appear. The *Practitioner* occupies a high position in medical literature, and has fulfilled a useful function; but such a review is almost enough to make Anstie turn in his grave, and the sooner the *Practitioner* disavows sympathy with such works the better for its future reputation.

BREACH OF PROMISE.

IN cases of breach of promise of marriage law and equity seem as far asunder as the poles. Marriage is a contract between two parties, and in ordinary cases of contract if *either* contracting party fails to fulfil the conditions there is ground for an action for damages. Marriage, however, in the eyes of a British jury, differs from other contracts in this—that actions for damages only lie against the male contracting party. It is true that a gentleman recovered £5 not long since from a faithless lady, but that is one of those exceptional cases which merely proves a rule, and there can be no doubt that had the sexes of plaintiff and defendant been reversed the damages would probably have been multiplied by 1000. Again, there seem to be no conditions or circumstances which justify a breach of contract. Let us suppose that two neighbours agree together to meet on a certain day to effect the mutual exchange of a horse and a cow, and that such exchange has been agreed upon in writing. Let us further suppose that, before the day for exchanging arrives, the owner of the horse writes to the owner of the cow, and explains that his animal has since the contract become broken-winded and broken-kneed, and that therefore the exchange had better not take place. Under these circumstances the owner of the cow would applaud the honesty of his neighbour, and we can hardly imagine an action for damages because one party had refused to cheat the other. In marriage contracts, however, a different view is taken, and if one of the parties after the agreement should by accident or ill-health become non

habilis ad matrimonium, he is still obliged to pretend at least to fulfil his part of the agreement, although grounds for divorce might exist the minute the conjugal knot was tied.

A case of this kind was lately tried at Abergavenny, in which a "lady" had brought an action because the gentleman to whom she was engaged had broken off the engagement on the plea of his own ill-health. It was proved that the defendant, who was an attorney's clerk, was suffering from phthisis, and the jury, instead of applauding him for refusing to gratify his affections at the expense of the happiness of a woman, and possibly of posterity, made him pay £200. Looking only at the equity of the case, we are inclined to ask, if a man be cast in such heavy damages for acting honourably, what ought he to have been fined had he selfishly fulfilled his contract?

THE ARCTIC MEDICAL SERVICE.

MR. CLEMENTS R. MARKHAM has published, under the title of "The Arctic Navy List, or a Century of Arctic and Antarctic Officers" (Griffiths & Co.), a list of the officers who served in the various Arctic expeditions from 1773–1873. He briefly describes the services of each officer, and the part which he played in the expedition or expeditions with which he was connected; and, at the close of the work, he adds a list of the officers about to be engaged in the expedition of the present year, with an account of their services. The work is singularly interesting, and to us it has a special value, as it exhibits the honourable services rendered to the expeditions by their medical officers very prominently. Perhaps this is most markedly shown in the numerous instances in which the names of medical officers have been given to newly-discovered localities in the Arctic regions. A list of these can hardly fail to be acceptable to our readers. Anderson's Bay, or Victoria Land, near Cambridge Bay, was named after R. Anderson, surgeon of the *Investigator* (Bird) in 1848–49, and of the *Enterprise* (Collinson), 1850–55. Armstrong Point, on the West Coast of Prince Albert Land, was named after the present Director-General of the Navy Medical Department, Sir Alexander Armstrong, K.C.B., who was surgeon of the *Investigator* (M'Clure), 1850–54. The Crimson Cliffs of Beverley (near Cape York, Greenland), Beverley Inlet, on the south coast of Melville Island, and Beverley Bay, on the north coast of Spitzbergen, were named after J. C. Beverley, who was assistant-surgeon in the *Isabella* (J. Ross), 1818, and in the *Griper* (Liddon), 1819–20, and was surgeon in the *Hecla* (Parry, 1827). Bradford Point, north-east point of Melville Island, was named after Abraham Rose Bradford, surgeon of the *Resolute* (Austin), 1850-51. Mr. Bradford is a retired Deputy-Inspector of Hospitals; he led a sledge-party to the east coast of Melville Island, and was away eighty days, the expedition traversing 669 miles. Cape Fisher, on the west shore of Hecla and Griper Bay, was named after Alexander Fisher, assistant-surgeon of the *Alexander* (Parry), 1818, and of the *Hecla* (Parry), 1819–20. Goodsir Inlet, on the east coast of Bathurst Island, was named after Anstruther R. Goodsir, surgeon of the *Lady Franklin* (Penny), in search of his brother, Harry Goodsir, previously curator of the Edinburgh Museum, who was assistant-surgeon of the *Erebus* (Franklin), 1845–48, and was lost with the expedition. Lyall Point, on the north-west of Bathurst Island, was named after David Lyall, assistant-surgeon of the *Terror* (Crozier), in the Antarctic Expedition, 1839–43. McCormick Bay, on the east coast of Wellington Channel, was named after Robert McCormick, assistant-surgeon in the *Hecla* (Parry), 1827, and surgeon and naturalist of the *Erebus* (J. C. Ross), in the Antarctic Expedition, 1839–43. Cape MacLaren, the west entrance of Lyon Inlet, on the coast of Melville Peninsula, was named after A. MacLaren, assistant-surgeon of the *Hecla* (Lyon), in 1821–23. Port Neill, south of Port Bowen, on the east coast of Prince Regent's Inlet, was named after S. Neill, surgeon of the *Hecla* (Parry), 1824–25. Skeoch Bay, on the south coast of Cockburn Island, was named after J. Skeoch, assistant-surgeon in the *Terror* (Parry), 1821–23. Sutherland Island, on the east coast of Wellington Channel, was named after Peter C. Sutherland, surgeon of the *Sophia* (Stewart), 1850–51. Toms Point, at the north-west end of Bathurst Island, was named after F. G. Toms, assistant-surgeon in the *Assistance* (Belcher), and the *North Star* (Pullen), 1852–54.

THE VIVISECTION CONTROVERSY.

THE new number of the *British and Foreign Medico-Chirurgical Review* leads off with an article under this heading. The writer forcibly illustrates the inconsistencies into which some of its indiscriminate opponents have fallen, by detailing the sufferings to which the lower animals are subjected under certain circumstances by sportsmen and epicures in the language that would have to be employed in describing those sufferings if they occurred in the course of so many experiments. And every thoughtful and humane man must recognise that the aggregate amount of physical suffering inflicted under such circumstances is very large. Thoughtless people are apt to lose sight of this altogether and to forget, in their anxiety to put down vivisection, that they are scarcely warranted in casting the first stone at them so long as so many cruel sports and practices remain to be got rid of. We have endeavoured to regard the subject of animal experimentation conscientiously and temperately, and with every desire to discover the limits within which it is justifiable, and we have suggested that the matter should be taken up and considered by a properly constituted commission. The writer in the *British and Foreign Medico-Chirurgical Review* considers the rules laid down by a committee of the Section of Biology, at the meeting of the British Association in 1871, good in principle, but wanting in authority, and suggests that the Royal Society, for example, might render an important public service if they were to consider and publish a decisive opinion and direction on this question.

A GOOD OPENING.

THE Axminster Guardians advertise for a district medical officer for Worle (area 5000 acres), at an annual salary of £40, the officer to furnish physic as well as attendance. The late incumbent of the post, whose salary was £30, has resigned, it appears, on account of the insufficiency of the remuneration for the duty to be performed. An advertisement of the guardians for a successor at the same salary was unanswered, and at a recent meeting many of them, it seems, were by no means astonished at this. Several protests were made against the preposterous rate of remuneration, having regard to the work which the medical officer was called upon to do. One guardian stated that "he had received direct information from previous medical officers of the same district that the appointment had not paid them for their shoe-leather. One had been part and parcel of the parish, and had not cared what he did for the love of the poor, all of whom he had known for many years, and another had been influenced by the same feeling. As, however, the late incumbent had asked for an increase to £50, he would suggest that the difficulty might be met by offering that gentleman a compromise of £40 per annum." There was a general feeling among the guardians that the doctor was underpaid, and some uneasiness as to what might happen if there were any pauper sickness before the appointment was filled up. To admit the insufficiency of the salary was

to admit the justice of the late incumbent's application for an increased salary, and it might be thought that the natural and proper course was to induce him to resume the appointment by offering him an increase. But the chairman, a "Reverend," it would appear, strange though it may seem, opposed such a course, observing "for his own part he did not like now to offer £40 per annum, as it bore so close a resemblance to going down on their knees; and, viewing matters in that light, he would oppose the resolution by moving an amendment to the effect that the salary offered be £30, as before." A guardian "then proposed, as a further amendment, that the salary should be £20 per annum." Another guardian added, "and I will propose that we advertise for some retired physician, who wants a little work to fill up his time, with no pay." After some further observations the question was submitted to the vote, when the numbers were 13 for £30, and 18 for £40 per annum. Advertisements were ordered to be prepared accordingly.

The *Weston Mercury*, from which journal we take the foregoing particulars, thinks that the proceedings of the Axminster guardians in this business "reveals a somewhat sorrowful expression of our modern civilisation"; and concludes that "either the lives of the poor, or the services of medical men, or both, are not held in the highest esteem."

THE SALE OF DISEASED AND PUTRID MEAT.

SINCE the appointment of medical officers of health and inspectors of nuisances all over the country prosecutions for the sale of diseased and putrid meat, judging from the provincial journals, appear to have considerably increased. This was to be looked for, having regard to the large quantities of such meat which found their way into the market, and for which a sale has been very commonly sought in localities where there was, before the passing of the Public Health Act of 1872, little chance of detection. Judging from certain recent cases, there is likely for some time to come to be a considerable divergence of opinion between medical men and veterinary practitioners as to whether certain classes of meat are unwholesome. In two instances now before us, in one of which a carcass was seized by the officers of the sanitary authority on account of multiple abscesses in the lungs, in the other on account of extensive tubercular disease of the lungs and abdominal organs, a great conflict of opinion took place between the medical witnesses, on the one hand, and the veterinary witnesses. The medical witnesses maintained that it was to be inferred, from the extent and state of the disease found, that the flesh of the animal would be unwholesome, and they pointed out that the flesh in each case had neither the appearance nor the consistency of thoroughly sound meat. The veterinary witnesses maintained that the extent and state of the disease discovered was not uncommon in horned beasts, that the change supposed to have been observed in the character of the flesh was not so great as to exclude it from the category of wholesome meat, that mischief to the health of the consumers of such meat was unheard of, and that if tubercular disease of the lungs and the intestines, and the so-called "multiple abscesses" in the lungs, were to be held as sufficient to condemn a carcass, the effect upon the meat supply of the country must be most disastrous. While in principle the medical witnesses were quite right, it must be confessed that the amount of evidence as to the injurious effect of the meat from animals suffering from the diseases in question is not large. But there is good reason to believe that the meagreness of this evidence really arises in great part from the want of sufficient opportunities for observation which have hitherto existed, and we may anticipate that the new medical officers of health will in time be able to clear up some doubts on this score. On the other hand, the veterinary evidence expresses the popular conceptions of the matter. It will behove medical officers of health to proceed carefully in this portion of their duty, and especially to seek for additional information as to the effects of meat of the kinds referred to upon the health of the population. In the case of multiple abscesses of the lungs the magistrates held that the unfitness of the carcass for food was not proved; in the case of tubercular disease the magistrates confirmed the seizure and condemnation of the carcass.

THE BELFAST DISPENSARIES.

WE exceedingly regret that an attempt to appoint additional medical officers to the dispensary districts in Belfast has failed. A motion to this effect was brought forward at a special meeting of the Committee of Management of the Belfast Dispensary District, on the 16th ult., by Dr. Wilberforce Arnold, who supported his motion in a powerful argument that was not at all answered by his opponents. He showed that the number of officers is the same now as it was twenty years ago, notwithstanding the enormous increase of population and the great addition to the duties of dispensary doctors made by recent Acts of Parliament, such as that of vaccinating, registering vaccination, registering births and deaths, and acting as sanitary officers under the Sanitary Acts. A very great point made by Dr. Arnold was that attention to the diseases of the poor required more time than used to be necessary for this purpose. Finally, he showed that within a few years three dispensary officers had died of over-work or fever. The old answer to this was forthcoming, that men were to be had on existing terms. This is a miserable and inadequate answer. Those who are responsible for medical attendance on the poor should remember the care which they take in regard to attendance on themselves and their families—viz., to have it good in kind and in amount. Dr. Arnold based his motion on the reports of the Local Government Board inspector and of the honorary secretary. With such arguments and such authorities on Dr. Arnold's side, the Belfast Committee, in rejecting his motion, must be charged with making inadequate provision for the onerous work to be done in its districts; and it is to be hoped Dr. Arnold will soon return to the subject.

SANITARY CONDITION OF FOLKESTONE.

THE reply made by Mr. Sclater-Booth in answer to the question put by Sir Edward Watkin last Tuesday night with respect to the sanitary condition of the town of Folkestone, must be deemed satisfactory. It appears from the report of Mr. Bateman, the medical officer of health, "that in the years 1873 and 1874 there were only four fatal cases of fever and scarlatina, and in the first three months of the present year no case of fever at all, in a population of 13,000, while the average mortality in past years has been under 17 per 1000.

In consequence of the statement recently made by Lord Robert Montagu in the House of Commons "that Folkestone might be matched with any town in England for abominable pestilential holes, from which scarlatina and fever are never absent," our Sanitary Commissioners visited the town, and have reported upon its sanitary condition. This report we will in due time lay before our readers, and the subject is one of special interest to the profession and to the public generally. In the meantime we would state that there is nothing in the present condition of Folkestone to excite alarm, that the town is perfectly healthy, and that the local authorities are doing all in their power to maintain the high standard that Folkestone has hitherto enjoyed among the southern watering places.

WOMAN'S HELP IN SANITARY MATTERS.

IT is always satisfactory to hear of systematic efforts being made to educate the poorer and more ignorant classes in matters appertaining to health and elementary physiology; and it is especially so to find that this field for sanitary labour is beginning to be recognised as one in which educated women may do useful work which is much needed. It is woman that has the ordering of a poor man's home—in how many thousand cases only a single room,—and woman's influence in bringing into these homes a conviction of the necessity for ventilation and cleanliness is far more likely to be successful than the most persevering efforts on the part of health officers or nuisance inspectors. The National Health Society have organised a course of lectures on these subjects in the East of London, and the first of the series was recently given in the Parochial Institute connected with the parish church of St. George-in-the-East, by Miss M'Cornish. This lecture was attended by more than 150 women, among whom were the nurses attached to the East London Nursing Society. The subjects dealt with were principally the importance of ventilation and pure air, the lecture being illustrated by a few experiments showing the poisonous nature of the carbonic-acid gas which prevails in excessive proportion in all crowded and unventilated rooms. The audience is described as having been attentive and evidently instructed. The success of the lecture probably led to its repetition in the school-room attached to St. Philip's Church, Stepney. The National Health Society cannot well engage itself in work more likely to further its objects than in promoting similar lectures in all the poorer neighbourhoods of large towns.

BEGGARS' BUSH BARRACKS.

THE Secretary for War replied at some length to a question by Sir L. Palk as to the occurrence of cases of enteric fever and scarlatina in connexion with sanitary defects in these barracks, the existence of which had been brought to the notice of the authorities in 1873, and could then have been remedied at a cost of £15. It will be remembered that some cases of enteric fever were reported as having occurred some months ago among the officers of the regiment of Foot Guards stationed at Dublin. It was not clear from the published accounts whether Captain Van de Weyer, for example, who was one of the sufferers, contracted his disease in the barracks or elsewhere. Mr. Gathorne Hardy stated that altogether four cases of enteric fever had occurred, and that it was still open to question whether they were attributable to the presence of insanitary conditions in the barracks. From the details he furnished to the House it is clear, however, that all the four cases had this history in common—viz., that they had all been exposed to the same alleged cause; and Mr. Hardy took care to add that additional ventilation for the drains had been authorised, although he declared the cesspits complained of in 1873 had been done away with, and replaced by waterclosets in March, 1874.

IMPRISONMENT IN JERSEY.

THE answer of the Home Secretary to the question of Mr. David James Jenkins, the member for Falmouth, touching the treatment of Harriet Gilbert, a girl of fourteen years, in the Jersey prison, unhappily leaves intact all the painful accounts of this case which we have published—viz., that although in the process of dying, she was kept in solitary confinement, on bread and water, for half the month of her imprisonment, including the period immediately prior to removal to the hospital, which she just reached in time to die, and that her death was accelerated by her treatment in prison. The Home Secretary did not allude to the admitted fact that in this instance she was not the real culprit, whatever criminality can have attached to one of fourteen years. We have no information that she received any nursing. The doctor of the prison has defended the bread and water diet and the general treatment of the dying girl, on the ground that it was doubtless much better than her ordinary diet. We look to the doctors of prisons to instil higher views than this, of what even criminals require, especially when they are children and the subjects of mortal illness. The case is not creditable to any concerned, and we can only hope that the assurance of the Home Secretary will be realised that such a case shall not occur again.

THE MEDICAL OFFICER OF HEALTH AND MORTALITY RETURNS IN BERMONDSEY.

THE sanitary state of Bermondsey has lately been unsatisfactory, and, among other diseases, diphtheria has prevailed to some extent. At a recent meeting of the Vestry, Dr. Parker, the medical officer of health, called attention to the awkward position in which he was placed by being deprived of the use of copies of the death register which he used to get from the Registrar-General, but which the Vestry now refuse to pay the registrar for furnishing. A long discussion took place on the subject, and in the end the matter was referred to the General Purposes Committee. The vestries cannot well be blamed for attempting to shift the payment for these returns off their own shoulders, but that while they are squabbling and fighting over such a petty source of expense they should allow their health officer to be deprived of the use of these valuable returns for three whole months is a serious dereliction of their duty as the sanitary authority. Health officers must have these returns, and should even receive daily information of deaths from epidemic diseases. We fear that Bermondsey is not the only district of London in which "on principle" the vestries still refuse to pay for these returns, and health officers are left to fight disease without their assistance.

INFANTILE MORTALITY.

A HIGH rate of infantile mortality has for a long time prevailed throughout the country. It is a subject deserving the closest attention of the profession, and especially demands all the vigilance of medical officers of health. In Sunderland last year the deaths of children under five years of age contributed close upon 50 per cent. of the total mortality. That many of these deaths could have been prevented by judicious feeding and care cannot be doubted. Dr. Yeld found on inquiry that, of 102 fatal cases attributed to the commonest disorder incidental to childhood, no less than 71, or over two-thirds of such children, had been fed by the bottle—an injurious system of rearing which should be strongly reprobated. Dr. Yeld infers either that less regard is now paid than heretofore to the sacredness of infant life or that more mothers than in former years are either constitutionally unable to nurse their children, or are unwilling, even for a few months, to forego their wonted ease in order to devote the requisite care and attention to their offspring.

AN ATTEMPT TO PROCURE EXAMINATION-PAPERS.

A CURIOUS and important case has arisen out of an alleged attempt on the part of a young man, named Samuel Cowap, to get possession of an examination-paper of the Pharmaceutical Society. It appears, according to the evidence at Bow-street, that he had incited one George Austin, who was in the employment of the firm that prints the *Pharmaceutical*

Journal and the examination-papers of the Society, to steal one of these papers for the purpose of affording an intending but backward candidate the benefit of perusing the questions beforehand. Acting under instructions, a copy of the questions was handed over to Samuel Cowap in exchange for half-a-sovereign; but a detective soon afterwards took the unhappy youth into custody, and he has been remanded bail being accepted. As Mr. Douglas Straight, who prosecuted, remarked, in asking for a remand, it was important to inquire further into the matter, as, if these examinations could be got up beforehand, there would be no end to the poisonings committed by ignorant chemists' assistants.

THE FACULTY OF GLASGOW AND THE UNIVERSITY.

THE Faculty of Physicians and Surgeons of Glasgow have voted a donation of 500 guineas to the funds of the Glasgow Western Infirmary. This is a liberal contribution to the local means of clinical education, but the gift is especially noteworthy in another point of view. For more than half a century there existed a state of chronic hostility between the Faculty and the Glasgow University, originating in circumstances that led to a protracted course of litigation, and perpetuated by the action of conflicting interests. The feelings which underlay these contentions, though occasionally breaking out afresh, have of late years been dying out, a good proof of which is afforded by this handsome contribution by the Faculty to the funds of the hospital so intimately connected with the University.

HEALTH OF BRIGHTON.

THE vital statistics of the popular Sussex watering-place for the year 1874, published within the last few days by Dr. Taaffe, medical officer of health, when compared with the returns of other similar resorts, place the health of the town in a very favourable light. Brighton has numerous sanitary advantages for maintaining a low rate of mortality: in addition to its fine sea front, it is sheltered from the north-east winds by the downs, the soil is dry and chalky, it has good natural drainage, its sewage is carried out into the sea and discharged at a point four miles away from the town, and it has an abundant supply of pure, wholesome water. Great improvement has lately been wrought in the condition of common lodging-houses and slaughterhouses, and provision has been made for the isolation and treatment of contagious diseases. We are glad to learn that the authorities have determined to plant as many trees as possible in the streets and open places of the town. For years it has been glaringly deficient in this respect, and it may be remembered that Dr. Samuel Johnson, when he visited Brighton, complained that if a man wished to hang himself he would not be able to find a tree on which to perform the ceremony.

TANCRED STUDENTSHIPS IN PHYSIC AT CAIUS COLLEGE, CAMBRIDGE.

THIS Whitsuntide there will be vacant two Studentships in Physic at Caius College, Cambridge, and the election to them will take place on Wednesday, the 19th of May next. These studentships, which are four in number, were founded by Christopher Tancred, Esq. Each student receives £100 yearly until the degree of Bachelor of Arts or Bachelor of Physic is obtained, and for three years afterwards. The candidates must be natives of Great Britain (i.e., actually born in England, Scotland, or Wales), members of the Church of England, unmarried, and between the ages of sixteen and twenty-two. The electors are the Master of Caius and Christ's Colleges, the President of the Royal College of Physicians, the Treasurer of Lincoln's Inn, the Master of the Charterhouse, and the Governors of Greenwich and Chelsea Hospitals. Information respecting the mode of application may be obtained from B. J. L. Frere, Esq., 28, Lincoln's-inn-fields, London.

MODERN TREATMENT OF LUNACY.

IT may, we think, safely be asserted that in no other country in the world are the insane so well treated and cared for as in England. Everything that skill and humanity can suggest is being done to alleviate the condition of the inmates of our asylums. Amusement as a curative measure in the treatment of lunacy has long been recognised by medical superintendents of county and private institutions. We are glad to notice that a most successful entertainment was given last week at the West Riding Asylum, of which Dr. Crichton Browne is the able director. The play chosen on the occasion was "Pygmalion and Galatea," in which the author, Mr. W. S. Gilbert, took a prominent part. The play had a long run in London, and is probably familiar to most of our readers. Among the large audience there were over a thousand lunatics, whose orderly and appreciative conduct during the performance was remarkable. The evening was brought to a close by a ball, in which visitors, lunatics, attendants, and nurses participated. The entertainment throughout was an unequivocal success, and reflects great credit on the management.

ABORTION-PRODUCERS.

AT the Liverpool Assizes a few days ago Alfred Thomas Heap, a man practising as a surgeon at Manchester, but having no diploma, was found guilty of murder for having caused the death of a young woman by endeavouring to procure abortion by using a sharp instrument. Some of our contemporaries, in alluding to this case, speak of the act as done by a surgeon. They would be entitled to our gratitude if they would be careful to find out, in publishing such cases, whether the actors are surgeons or qualified medical men, or only quacks, without either diploma or respectability.

AT LAST.

THE Local Government Board having sent a third intimation to the Cambridge Sanitary Authority to appoint a medical officer of health, a meeting was held at the Guildhall, Cambridge, on Tuesday, to take steps in the matter. It was ultimately decided that a health officer should be appointed, and that his salary should be fixed at £250 per annum, with permission to practise in the town.

DIPHTHERIA AT WOOLWICH.

THE removal of the soldiers' married families from the cottages on the Common has not had the effect of eradicating the disease. We understand that nine cases of diphtheria have occurred among the women and children since they have taken up their new quarters, and that three of them have proved fatal.

A LONG struggle has been carried on between the Local Government Board and the guardians of Hackney respecting the necessity of erecting a separate infirmary for the sick poor of the union. The contest was marked by some degree of acrimony, and the proposed measure caused great ill-feeling in the parish. Eventually, but after many protests, the local was obliged to give way to the superior authority, who would not be moved from its determination. At a late meeting of the guardians a tender to build the required establishment at a cost of over £5000 was accepted.

IN a paper "On Food Adulteration and the Legislative Enactments relating thereto," read before a meeting of the Society of Arts, Mr. Wentworth Scott, public analyst to the counties of Derby and North Stafford, stated that he had officially examined 170 samples of intoxicating drinks, obtained in the latter county, with the result that a large proportion were found to be adulterated. The ales tested were sophisticated with cocculus indicus, than which a more pernicious adulterant cannot be found; they were also largely impregnated with salt, and occasionally contained colchicum. Some of the brandies underwent most complicated processes, in which a preparation of cayenne pepper played a considerable part, before being sold to the unfortunate consumers. Teetotalers will be interested to learn that "Good Templars' claret" is mainly composed of decoction of logwood, free sulphuric acid, and impure spirit containing much fusel oil. We are pleased to learn that convictions were obtained in many of these cases.

A BILL, bearing the names of Mr. Cowper-Temple, Mr. Russell Gurney, and Dr. Cameron, has been printed, which has for its object to "amend the Medical Act, 1858, so far as relates to the registration of women who have taken the degree of Doctor of Medicine in a foreign university." It proposes that women who have such degrees may be registered on the same conditions as are applicable to degrees granted by the examining colleges and bodies of the United Kingdom. What next?

THERE was a considerable increase in the rate of mortality in London last week. It reached 28 per 1000 annually, a higher figure than we have generally to record. The deaths amounted to 1873, including 1 from small-pox, 24 from measles, 32 from scarlet fever, 16 from diphtheria, 92 from whooping-cough, 30 from different forms of fever, 24 from diarrhœa, and 534 referred to diseases of the respiratory organs.

DR. C. H. RALFE has been appointed to deliver the Thruston Speech this year at Caius College, Cambridge, on the 11th of May, in commemoration of Dr. Wendy. Dr. Ralfe is a Master of Arts of the University, and holds the office of physician to the Seamen's Hospital, Greenwich. We congratulate Dr. Ralfe on the distinction conferred upon him by his Alma Mater.

OUR contemporary the *Western Morning News* states that "Hospital Saturday" is to be revived this year, though it failed so distinctly last. The final Saturday in July is chosen as the day for the collection. But if it does not lead to something more than last year the movement will be abandoned. It only gives people an excuse for not subscribing to hospital charities.

IT appears that the resolution of the Council of the College of Surgeons respecting the appointment of examiners under the Conjoint Scheme, to which we referred last week, was not, as we had been given to understand, passed unanimously, forasmuch as it was opposed by Professor Humphry, who gave his vote against it.

THE public analyst for Warwickshire reports that the Adulteration Act is working very satisfactorily in that county. Last year a large amount of good seems to have been effected. The analyst is, however, constrained to point out many defects in the Act.

WE understand that St. Mary's Hospital, Paddington, will shortly be closed for a period of two or three months in order to be thoroughly cleansed and put into a state of good sanitary order.

IT is stated that Surgeon-General J. C. Brown, C.B., chief of the Bengal Medical Department, will retire at the end of the present year, after having been forty years in the service. In 1861 he received the appointment of Honorary Surgeon to Her Majesty, the tenure of which will not be affected by his retirement.

MR. J. F. BLAKE, M.A. Cantab., has been appointed Lecturer on Comparative Anatomy and Zoology at Charing-cross Hospital, vice Professor Garrod resigned. Mr. Blake is a scholar of Caius College, was senior of the Natural Science Tripos in 1862, and is favourably known as the author of "The Catechism of Zoology."

DR. WINTER, Inspector-General of Hospitals and Fleets, now at Plymouth, has been placed on the retired list, having reached the age of sixty years. Dr. Winter has well earned his repose.

WE regret to learn that one of the most able and distinguished students of St. Thomas's Hospital, Mr. Fredk. H. Peck, has just died of typhoid fever in the hospital.

DR. C. THEODORE WILLIAMS, F.R.C.P., has been appointed by the Council of the Medical Society of London to deliver the Lettsomian Lectures in January, 1876.

DISCUSSION AT THE OBSTETRICAL SOCIETY ON PUERPERAL FEVER.

THE following is a brief outline of the discussion which was opened at the Obstetrical Society on Wednesday last, of which a full report will appear in our next number. The attendance of members and visitors was very large, and the debate was continued for the space of two hours. The speakers of the evening were chiefly those who had come from a distance to be present at the meeting, and others who are not actually engaged in obstetric practice. It will be seen that there was a singular unanimity of opinion among the speakers as to the non-specificity of puerperal fever.

MR. SPENCER WELLS, after a few introductory remarks upon the value of such discussions, opened the debate by limiting himself strictly to the series of questions which have already been published (*vide* THE LANCET of March 27th, p. 453). Quoting Dr. Farre's definition of puerperal fever as contained in the Nomenclature of Diseases published by the Royal College of Physicians, he laid stress upon the fact that local lesions often, but not always, accompanied the fever; that the poison might be so concentrated, or taken in so large a quantity, as to kill before local lesions could be developed, or else that the dose or virulence of the poison was so small as to lead to its elimination before time had been given for the establishment of such lesions. In treating of the first three queries, Mr. Wells did not bind himself to accept any dogmatic statement as to puerperal fever being a distinct entity, but simply propounded the questions to be left for discussion by the members. His remarks, which were commendably brief, concluded by his recalling certain facts, which he had published twelve years ago, on the causes of mortality after surgical operations, where, applying Pasteur's (then recent) discoveries in fermentation to explain pyæmic and other processes, he had proposed the use of sulphurous acid and the sulphites as a means of protecting tissues from the action of the supposed virus. Two years later Professor Lister introduced his antiseptic treatment, grounded on the germ theory of septic processes, and, looking to the vast results obtained by Lister in ridding hospitals of pyæmia &c., he thought that, with similar precautions, lying-in hospitals could be

rid of puerperal fever. As an encouragement to this end, he pointed out what had been done in rendering ovariotomy successful by the simple adoption of sanitary measures.

It was then announced that letters had been received from several gentlemen who were unable to attend, among them Dr. Robert Lee, Professor Simpson (of Edinburgh), Professor Matthews Duncan (whose letter condemnatory of the recent conviction of midwives for manslaughter was read), and many others.

The President (Dr. Priestley) invited remarks from country practitioners who might be present, and called upon Dr. LEISHMAN (of Glasgow) to speak. Dr. Leishman's speech was chiefly remarkable for the frank and open manner in which he announced that he had been led to form conclusions upon the nature of puerperal fever different from those which he had already published; for he has come round to the belief that the various forms of fever included under that term are all or nearly all of a pyæmic or septicæmic character. For, however different in their starting-point, whether it be from scarlet fever contagion, or whether from local inflammations, the later symptoms in all forms are of the same septicæmic character. He thought that the statement that animal poisons from any source could engender the fever should be limited. Puerperal fever in a lying-in hospital is a greater scourge than pyæmia in a surgical hospital; so that it might well be said that a woman who entered the former institution simply to fulfil a physiological function was really exposed to as great dangers as if she were exposed to typhus.

Dr. NEWMAN (of Stamford), in an admirable speech, formulated certain propositions founded upon the results of twenty years' experience in country practice. That there is no such thing as a definite puerperal fever, strictly so called; that a large number of cases of the disease do undoubtedly owe their origin to the exposure of the patient to some definite infective fever (and he quoted a fatal case, in which the only possible source was to be found in a child having scarlet fever at a house where the patient paid a visit shortly before her confinement); that in other cases local inflammations lead to the development of a strictly pyæmic state; that, in his opinion, lowered vitality from exposure to sewer-air in badly-drained houses greatly predisposed to the infection; that possibly the greater activity of the vital processes in a parturient woman accounted for the readiness with which they were infected by poisons against which they would be proof in the non-parturient state; finally, he thought that mental disturbance acted as a great predisposing cause.

Dr. BRAXTON HICKS followed in some remarks upon the necessity of having a clear idea of the clinical position of puerperal fever. A very large proportion of cases were due to contagion from the specific fevers or from decomposing materials in the uterus, to which must be added those in which mental influences had a share,—in this way but 10 or 11 per cent. alone remained the origin of which it was difficult to trace. Sometimes a parturient woman exposed to scarlatinal poison does not take the fever; it would be interesting to know if in such a case the patient had already had scarlet fever. That decomposing clots do give rise to the symptoms of the disease was undoubted, and on their removal and washing out of the uterus the symptoms subside. Dr. Hicks concluded his remarks by stating that there was no doubt as to the contagiousness of puerperal fever, however it be produced, but he questioned if the mere aggregation of puerperal patients in a lying-in ward was *per se* sufficient to give rise to the disease.

Mr. JONATHAN HUTCHINSON, having been called upon, contented himself with giving, in his clear and cogent style, his definitions of "erysipelas," "septicæmia," and "pyæmia." We are unable here, from want of space, to do full justice to his valuable remarks. Suffice it to say that he demonstrated that erysipelas, which is so potent an agent in producing puerperal fever, is *not* a specific fever; that it is simply a local inflammation, to which all its febrile and general symptoms are superadded—a view supported by its unilateral character, by the absence of any incubative period, by it being possible to cut short its extension by appropriate treatment. In this respect it resembled inflammation of the uterus. The term "septicæmia," he said, should be limited to poison introduced into the blood from the patient's own tissues, which may have been primarily inflamed by irritants derived from without, or even without any external cause whatever; while, as to "pyæmia," he fully believed in its sole origin in a suppurative phlebitis.

Dr. B. W. RICHARDSON, after showing that the parturient state was physiologically one of great peculiarity, and prone to the development of febrile conditions, proceeded to answer each of the questions propounded by Mr. Wells. He believes that there is no special "puerperal fever"; that it may simply be a fever of reaction or resistance, such as follows amputation of a limb; or that, from the peculiar condition of the blood, it may lead to cardiac thrombosis; or that it may be of the type of a "bilious remittent" fever; or of septicæmic nature from some local injury inflicted during labour; or, finally, that it may be due to poisons derived from without. With regard to the relation of bacteria, &c., to the pyæmic process in the puerperal state, Dr. Richardson said, that for twenty-five years he has always held that septic processes are purely physical or chemical, the association of organic forms being a coincidence, without causal relation; and that antiseptics act solely in a chemical way upon these septic processes. At the conclusion of Dr. Richardson's remarks, the debate was adjourned on the motion of Dr. Barnes.

Correspondence.

"Audi alteram partem."

A NEW TRUSS PAD.

To the Editor of THE LANCET.

SIR,—In August, 1873, I described a new kind of truss pad, which consisted essentially of a hollow pad, in the centre of which was a spiral spring—in fact, a Cole's or a Teale's truss, having the body spring attached to the convexity or convex surface of the pad plate, instead of to the spiral spring in its concavity. The effect of this transposition is that the chief resistance of the pad is transferred from its central and most convex part to its circumference; and whereas in the above-named trusses the surface of the pad which is applied to the skin undergoes no alteration of form under any circumstances, but only of inclination, that of the pad in question becomes changed, under the impulsion of a rupture, from a convex or flat to a concave form. To ensure this result, however, it is necessary that the pad spring should be a little weaker than the body spring, so that, when the patient coughs and the hernia is thrust outwards, the pad, instead of rising *en masse* or at some part of its border, yields only at its centre, while its circumference is kept firmly in its situation by the stronger resistance of the body spring.

A pad of most ingenious construction, similar in principle to the one just described so far as the action of its centre is concerned, but differing from it as regards the action of its circumference, has been recently invented and patented by Dr. Schoevers, of the Hague, and promises to be superior in some respects to the one above alluded to. It is composed of (1st) a certain number of springs arranged in the form of

a circle or of an ellipse. (Fig. 1.) In the centre or point of union of these circular springs, which in a state of rest form a cavity, there is attached (2ndly) a conical spring, furnished

with a metallic plate (Fig. 2), which is only brought into action when the circular springs are expanded by means of

the pressure of the body spring. The above parts are enclosed in (3rdly) a vulcanised india-rubber capsule (Fig. 3),

which yields to the expansion of the circular springs, and also serves to protect them and to complete the pad. Thus constituted, it is connected to the body spring—which is of the half-circle type and of French curve—by means of the ball-and-socket joint seen on the back of the pad in Fig. 2; and the instrument is completed by a connecting strap, which buttons on to a stud at the extremity of the body spring.

The specimen which has been sent me from the Hague is a No. 36, and, notwithstanding that the body spring is very strong, the instrument is so light that it weighs under four ounces. It may also be as well to mention that, although the connexion between the pad and spring is by a ball-and-socket joint, it is not, like Salmon's, which is similarly connected, opposite-sided.

As regards the value of this truss for the retention of difficult ruptures I cannot at present speak authoritatively, not having made trial of it; but, looking to the action of the elastic and expanding arms which form the circumference of the pad, it seems to me likely to have a firmer grip of the parts against which it may be placed, and consequently a greater retentive power than that of any other pad with which I am acquainted. It should be mentioned that the instrument is sold by Messrs. Maw, Son, and Thompson, of Aldersgate-street, who, I believe, are the sole importers.

I am, Sir, yours obediently,

March, 1875. C. HOLTHOUSE.

THE BIRMINGHAM MEDICAL INSTITUTE.

To the Editor of THE LANCET.

SIR,—I avail myself of your columns to tender my acknowledgments to 460 members of the profession residing in Birmingham and its immediate neighbourhood who, to this date, have placed on record their opinion as to the propriety of the admission of professed homœopathists to the membership of the Birmingham Medical Institute, by returning to me my circular on this subject with their signatures. Of this number, 436 say, without reservation in their judgment, that "professed homœopathists, so long as they assume a mode of practice and maintain a name calculated to mark them from the general body of the profession," should not be admitted; 19, equally without reservation, say they should; and 5 desire to be neutral.

I venture to think that here is the "voice of the profession" in our midst on the "difficult question," and a justification of my protest in the shape of an amendment to the approval of the report of the committee which passed at the recent annual meeting, to the effect that it was to be regretted that the opinion of the profession had not been sought ere donations from professed homœopathists had been received, and their admission determined, only by a committee.

Sir, the loss of my amendment by a majority of twenty-nine, in a meeting composed in the main of members on whose attendance unusual influences had been brought to bear, and who were led up to a vote by threats and denunciations so violent as to be without a parallel, is to me of little moment. I regard in the same light the arrogant and grossly personal attack I encountered at the hands of Dr. Heslop, so long as the true sentiments of our order are on record on this matter, and I therefore cheerfully leave his conduct to the judgment of the profession, as well as to that of his immediate associates, who, whatever may have been their dissent from my views, have one and all confined themselves in their utterances within the bounds of what is fair and proper in honourable controversy; and I can truly say that it has been my aim and object, from first to last, to avoid any references of a personal character, which are calculated only to weaken the best of causes, and to redound to the discredit of those who utter them.

I shall, however, indeed rejoice if my action in this matter leads to the abandonment of the name of "homœopath." If Dr. Dudgeon*, who has watched the recent struggle with no common interest, and has appreciated the force and fairness of the test I submitted, can say "that he holds himself free to avail himself of the resources of therapeutics, including homœopathy," and that the name "homœopath" is a "nick-name," and to be "abandoned," then my task is done, and I, for one, fully admit that he claims a consideration at our hands that ought not to be withheld, and manifests an example that, sooner or later, must be followed.

Lastly, let me say that, although I have steadfastly declined any correspondence on this purely medical question in our local press, and have sustained in certain quarters unmeasured abuse, I cannot but recognise the fairness and the wisdom of the comments made by the *Birmingham Daily Post* on the result of the recent meeting, and I take this opportunity to say, in accordance with the spirit of those observations, that the Institute may yet find myself, and, I trust, those who have acted with me, amongst the number of its best friends.

I remain, Sir, yours, &c.,

Birmingham, April 5th, 1875. OLIVER PEMBERTON.

LITHOTOMY AND LITHOTRITY.

To the Editor of THE LANCET.

SIR,—Sir Henry Thompson's interesting account of the good results of his last 100 cases of stone, together with his previously published cases, furnish a large body of evidence from which surgeons may extract most valuable information.

The whole series leave little to be supplied in the way of what I may venture to call—in no offensive sense—the rough statistics of stone. They indisputably establish their author's claim to an unrivaled experience and dexterity as a lithotritist, and they show to some extent the comparative mortality of the cutting and the crushing operation in the practice of one in whose dexterous hands all advantages that may belong to lithotrity have certainly been fully developed.

There can now be little doubt that in skilled hands and in properly selected cases of moderate-sized stones, the operation of lithotrity exposes those on whom it should be practised to a somewhat less risk of death than those must incur whose larger stones can only be treated by lithotomy. Each operation has, doubtless, its proper domain; still there are many cases which lie, as it were, on the border line, where it is difficult to decide between the respective claims of the two proceedings to a preference.

It is in view of such cases that those most interested in the matter now need further information, not as to the comparative fatality of the two operations, but as to the comparative condition of the two classes of survivors.

An impression exists, founded, so far as I know, on no sufficient data, that a patient who survives after being cured of stone by lithotomy is, with rare exceptions, secure from a recurrence of his malady, and is relieved henceforth from all his sufferings and discomforts. Whether this be so or not, a very interesting line of inquiry suggests itself—namely, the condition of patients some time (say a year) after the removal of stone, whether by lithotomy or lithotrity. This investigation would be difficult to carry out in the case of private patients, who are free to change their surgeons, and are especially prone to seek fresh advice should their unfavourable symptoms persist or their malady recur. Still with the assistance of the private medical attendants of calculous patients such an inquiry as I have suggested could be made, and it would prove a most valuable addition to our knowledge if those who have the charge of such as have been cured of stone, either by lithotomy or lithotrity, would record the condition of each patient when a full year has elapsed since the removal of the stone.

I am, Sir, yours, &c.,

April 7th, 1875. THOMAS SMITH.

* THE LANCET, April 3rd, 1875.

TUMOURS OF THE ORBIT.

To the Editor of THE LANCET.

SIR,—Allow me to make a few remarks on the discussion at the Royal Medical and Chirurgical Society, which occurred on the occasion of the reading of Mr. W. Rivington's paper "On a Case of Pulsating Tumour of the Left Orbit, consequent upon a fracture of the bones of the skull," as reported in the current number of your journal. I shall restrict myself to that part of the discussion which relates to orbital tumours, the diagnosis of which is notoriously difficult. Mr. Heath questioned the propriety of resting a diagnosis of aneurismal disease too exclusively on the bruit, as felt by the patient and heard on auscultation. Enough material has been gathered and recorded during the last dozen years, from the examination of patients and from post-mortem inspections, to assure us that perceptible pulsation in an orbital tumour, ascertained directly or through the medium of the eyeball, cannot be relied on as diagnostic of orbital aneurism.

Then followed Sir J. Paget's remarks in illustration of the cause of orbital pulsation, as might probably be gathered from the conditions of pulsation of tumours situated in bony cavities. He showed that a tumour of any sort in a bony canal may receive a considerable pulsation from bloodvessels of small size in the cavity or in its walls, &c. &c., and farther that the fat in the orbit, which is fluid at the temperature of the body, on which the eyeball floats freely, is a favourable medium for transmitting pulsations to the eyeball, when there is some disturbance in the orbital system of circulation. I wish, however, to draw attention to another fluid medium which operates in the same direction; I mean the accumulation of serum in the tunica vaginalis oculi—the capsule which surrounds the optic nerve and the eyeball. I am the more desirous of writing about it as the tunic is not generally described by anatomists, and the extensive bearing which it has on orbital pathology is scarcely recognised by surgeons.

In all cases of protrusion of the eyeball attended with orbital inflammation or orbital infiltration, the ocular tunic becomes more or less distended by serum, and sometimes to a considerable extent. There may be dropsy of the tunic from inflammation of the eyeball alone, but that does not concern us here. If the orbital fat exerts its fluid influence in transmitting pulsation to the eyeball, a like but still more applicable physical effect, looking at the subject comprehensively, is to be found in a bladder of fluid entirely surrounding the eyeball.

I am, Sir, your obedient servant,

Brook-street, W., April 8th, 1875. HAYNES WALTON.

Obituary.

THOMAS PAGET, F.R.C.S. ENG., LEICESTER.

THOSE who live near Leicester, or who lived in Leicester fifteen or twenty years ago, will be best able to judge of the kind of sensation produced in that town and county by the announcement of Mr. Paget's death. Few men were better known by name and fame and figure. Mr. Paget had been out of active practice for some years. His connexion with the Infirmary of Leicester terminated seven or eight years ago, and he had been very effectually succeeded, both as an operator and a consultant, by younger men, but he had been so long and withal so creditably before the public and profession of Leicester and of the Midland Counties generally, that his death occasions a sense of blank and change which is produced only by the death of men who have been exceptionally useful and conspicuous. By the public he will be long remembered as a good, clear-headed practitioner, especially in surgery. By the profession he will be remembered as an operator, whose work was characterised by coolness, precision, and success. No one in the county of Leicester twenty years ago in any surgical dilemma felt quite satisfied until he had had the benefit of the opinion of "Tom Paget," as he was often called. Mr. Paget will be remembered as one of the most successful lithotomists, to be named in the same rank with Martineau, Dalrymple, Crichton, and others. Of late years, in common, we believe, with other Leicester surgeons, he preferred the median operation in cutting for stone, to which his attention was drawn mainly by Mr. Allarton's pamphlet some twenty years ago. He had also a considerable reputation as an ophthalmic operator. His contributions to surgical literature were not numerous. He became a member of the College of Surgeons and of the Apothecaries' Hall in 1818, and an Honorary Fellow of the College in 1843. He had the great distinction of having been the first provincial surgeon elected to the Council of the College of Surgeons, though he never took an active part in its deliberations. He lived and died in the Unitarian faith, of which he was a principal supporter.

HERMAN S. MAY, M.D.

WE have to record the death of Dr. May, at his residence, No. 8, Summerlands, Exeter, on the 15th March, at the ripe age of eighty-six, after a brief illness. He was one of the oldest members of the College of Surgeons, having been admitted a member in 1810, and perhaps the oldest of the army surgeons. He was employed as assistant-surgeon at Plymouth, in 1809, to attend the wounded after the battle of Corunna, which appointment he obtained whilst a student at Guy's through the kindness of Sir Astley Cooper; but his commission in the regular army did not commence until 1810, when he was appointed to the 81st Regiment of the Line, and joined the expedition despatched from Sicily to the East of Spain, under the command of General Lord Bentinck, to oppose the operations of Marshal Suchet's corps, then in Valencia. He was present with his regiment at the sanguinary battle of Castella, in April, 1812, and at the siege of Tarragona. Subsequently, after the battle of Waterloo, he joined the Duke of Wellington's army in Paris, and remained with the army of occupation in France until the peace.

Dr. May practised successfully at Tiverton for twenty-five years, when he retired from general practice. He then proceeded to Edinburgh with his son, who entered the hospital and schools of that city. Here they both studied together, and after the expiration of three or four years his son (since dead) took the M.D. degree of Edinburgh, while he proceeded to St. Andrews, and became M.D. of that university. He finally settled at Higher Summerlands, Exeter, where he had received his early education.

Dr. May's father and three uncles were beneficed clergymen in the north of Devon. He died without issue surviving him. After numerous legacies to his family connexions, he has bequeathed £600 to the Exeter Hospital and Eye Infirmary.

Medical News.

ROYAL COLLEGE OF SURGEONS OF ENGLAND. — The following gentlemen passed the primary examination in Anatomy and Physiology at meetings of the Court of Examiners on Tuesday and Wednesday last:—

W. J. Pycock, E. J. Wood, O. Tottie, and H. Lighton, Cambridge; H. J. D. Innes, G. R. Turner, and T. E. Worgan, St. George's Hospital; W. J. Cant, W. Leah, C. L. Williams, C. Bradford, and A. Pratt, Birmingham; B. J. Newmarch, W. T. Miles, and E. Thurstan, King's College; W. S. O. Williams, Glasgow; P. A. Steedman, T. H. Gillam, A. R. Anderson, E. W. Walter, W. J. Haines, R. H. Schofield, St. Bartholomew's Hospital; R. Walker, C. F. Hawkins, M. Greenwood, E. Berdoe, and J. G. Clark, London Hospital; H. Davy, T. H. Berry, J. C. Uthoff, J. W. Meek, and P. Horrocks, Guy's Hospital; J. Wiglesworth, J. Rose, R. H. Jones, W. Gillibrand, and W. Barber, Liverpool; W. M. Hope, W. J. Frankish, E. Hughes, and E. S. Morgan, University College; C. K. Morris, F. W. Giles, and J. H. Battye, St. Thomas's Hospital; R. N. Hartley, Leeds; G. S. Griffiths, T. James, and F. Bellaby, Middlesex Hospital; A. Hemsted, Belfast; H. Whitehead and F. S. Boreham, Charing-cross Hospital; G. B. Fraser and W. S. Cox, St. Mary's Hospital; C. Henderson, Bristol; F. T. Willis, G. W. Sidebotham, J. Fergusson and Albert Andrew, Manchester.

APOTHECARIES' HALL. — The following gentlemen passed their examination in the Science and Practice of Medicine and received certificates to practise on April 1st:—

Hales, Robert Turner, Holt, Norfolk.
Hopkins, John White, Handsworth, Birmingham.
Maine, Walter, Francis-street, Bedford-square.
Moore, Charles John, Royal Dockyard, Devonport.
Sykes, John Frederick Jos., Fitzroy-square.

The following gentlemen passed the Primary Professional Examination on the same day:—

Brent, Mortimer John de, Westminster Hospital.
Popert, Adolph Joseph, London Hospital.

[The address of Mr. Edw. Joseph Day was in our last misprinted "Woodgates, Dorset." It should have been "Woodyates."]

ROBERT V. SKINNER, M.R.C.S. Eng., has been appointed Mayor of Winchelsea.

A THIRD CHAIR OF CHEMISTRY is to be created in the Faculty of Sciences of Paris, the necessary grant having been unanimously voted by the National Assembly. The new chair is to be devoted to organic chemistry—a branch of the science which, from the arrangements heretofore existing, has been kept somewhat in the shade.

A HANDSOME PAINTED WINDOW has been placed in the south aisle of St. Giles's Church, Norwich, to the memory of the late J. Godwin Johnson, F.R.C.S., who died Jan. 13th, 1874. The window is one of three lights, the subjects of which are—our Lord healing the lame man, Christ blessing little children, and the parable of the Good Samaritan. This window is erected by his old pupils as a token of their love and esteem for him. Mr. Johnson practised for upwards of fifty years in Norwich, where he was universally respected and beloved.

ILKLEY CHARITY HOSPITAL.—The annual report, read at the meeting of the friends of this useful institution a few days ago, was of a satisfactory character. It appears that the number of patients relieved at the hospital during the last twelvemonth exceeded that of any former year. Though there remains a balance in the hands of the treasurer, the need of increased pecuniary aid was represented as urgent. It is interesting to note that the hospital is designed chiefly for the restoration after sickness of those who are still able to take part in the active duties of life.

PRESENTATIONS.—Dr. Mackenzie, of Kelso, was recently presented by his numerous friends and patients with a valuable marble timepiece and a cheque for a sum of money. The timepiece bears the following inscription:—"Presented to W. M. Mackenzie, Esq., M.D., Kelso, with 500 guineas, by his friends and patients, on the completion of his twenty-fifth year as a medical practitioner in Kelso and neighbourhood, in testimony of the high estimation in which his professional and public services during that long period are held, as well as the regard and kindly feeling entertained for him by them. 26th March, 1875."—On Wednesday evening, the 24th ult., a number of the students of Anderson's University, Glasgow, presented A. M. Buchanan, M.D., M.A., Professor of Anatomy, with an address and a handsome ebony walking-stick, with a richly-chased gold head, bearing an inscription and monogram.

Medical Appointments.

BINGHAM, J. J., L.R.C.P.Ed., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for the No. 5 District of the Mansfield Union, vice Turner, resigned.

BOUGHTON, J. H., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for No. 6 District of the Upton-on-Severn Union, vice Cowley, whose appointment has expired.

BOYD, H. W. B., L.R.C.S.I., L.K.Q.C.P.I., L.M., has been appointed House-Surgeon to the Queen Adelaide Dispensary, Bethnal-green, vice Pocock.

BRAMWELL, B., M.B., L.R.C.S.Ed., has been appointed Pathologist to the Newcastle-upon-Tyne Infirmary.

BURROUGHS, E. F. H., M.R.C.S., L.S.A., has been appointed Medical Officer to the Middlezoy Friendly Society, and to the Prudential Assurance Company.

COWLEY, J. S., M.R.C.S.E., has been reappointed Medical Officer and Public Vaccinator for No. 3 District of the Upton-on-Severn Union.

DAWSON, Mr. C., has been appointed an Assistant Resident Medical Officer to the General Infirmary, Leeds, vice Atkinson, resigned.

EAGAR, R. T. S., M.B., M.R.C.S.E., has been appointed House-Surgeon to the Coventry and Warwickshire Hospital, vice Wybrants, resigned.

ELLIS, E. T. C., M.R.C.S.E., has been appointed Medical Officer for the Heathfield District of the Hailsham Union, Sussex, vice Press, resigned.

ELLIS, H. D., M.R.C.S.E., has been appointed Medical Officer for the Poole District, South-Western Railway, vice Crabb, deceased.

FAIRWEATHER, A. F. A., M.B., C.M., has been appointed Medical Officer for the Pocklington No. 2 District and the Workhouse of the Pocklington Union, Yorkshire, vice Deane, resigned.

FOULIS, Dr. D., has been appointed Pathologist and Curator of the Museum at the Royal Infirmary, Glasgow, vice Coats, resigned.

FOX, E. C., M.D., L.R.C.P.Ed., L.R.C.S.Ed., has been appointed Medical Officer for the Newton-Poppleford District of the St. Thomas Union, Exeter.

GOWLAND, Mr. G. R., has been appointed Resident Assistant Medical Officer at the Crumpsall Workhouse, Manchester, vice Tytler, resigned.

GRANDISON, A., M.B., L.R.C.S.I., L.M., has been appointed Medical Officer for the Workhouse and St. Mary's District, and Public Vaccinator for the first Division of the Dover Union, vice Walter, resigned.

GRIFFITHS, W. H., L.R.C.P.Ed., L.R.C.S.Ed., L.M., has been appointed Lecturer on Chemistry at the Ledwich School of Medicine, Dublin, vice Cameron, appointed Professor of Chemistry at the Royal College of Surgeons, Ireland.

HANDYSIDE, P. D., M.D., F.R.C.S.Ed., has been appointed substitute Examiner in Anatomy and Physiology at the University of St. Andrews, in consequence of the illness of Professor Bell.

HAY, F., M.D., M.S., has been elected Visiting Surgeon to the Hull Dispensary, vice Shackles, resigned.

JONES, G., L.R.C.P.Ed., L.F.P. & S. Glas., has been appointed Medical Officer to Court Framlingham Castle, No. 6083, of the Ancient Order of Foresters, Framlingham.

LATHAM, P. W., M.D., F.R.C.P.L., has been reappointed a Physician to Addenbrooke's Hospital, Cambridge, upon the expiration of his twelve years' term of office under the rules.

LEE, E., M.R.C.S.E., has been appointed an Assistant Resident Medical Officer to the General Infirmary, Leeds, vice Hawkins, resigned.

LYALL, W. L., M.D., C.M., of Glasgow, has been appointed Parochial Medical Officer and Public Vaccinator for the Parish of Campsie, Stirlingshire, vice Wilson, resigned.

M'CLURE, T., L.R.C.P.Ed., F.R.C.S.I., has been reappointed Medical Officer for No. 2 District of the Axbridge Union at an increased salary.

MINTER, J. M., M.D., has been appointed (additional) Hon. Physician to Her Majesty the Queen.

MORRELL, R. B., L.R.C.P.Ed., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for No. 5 District of the Bradfield Union, vice Kidgell, resigned.

POPHAM, T., L.K.Q.C.P.I., L.M., L.R.C.S.I., has been appointed Medical Officer, Public Vaccinator, and Registrar of Births &c., for the Bantry Dispensary District, and Medical Officer to the Workhouse of the Bantry Union, Co. Cork, vice Tuckey, deceased.

SANDERSON, C., L.R.C.P.Ed., has been appointed Medical Officer and Public Vaccinator for the Districts of Drymen and Aberfoyle, Stirlingshire and Perthshire.

SHEPPARD, H. H., M.R.C.S.E., has been appointed Medical Officer to the combined Friendly Societies, Harrogate, vice Milner, resigned.

SUTTON, F., F.C.S., has been appointed Public Analyst for Great Yarmouth: 10s. 6d., 20s., and 40s. per analysis, according to schedules.

WELSH, J., F.F.P. & S. Glas., has been appointed Medical Officer and Public Vaccinator for the Brampton-Brian District of the Knighton Union, Radnorshire, vice Brown, whose appointment has expired; also Medical Officer of Health for the Knighton and Brampton-Brian Sub-district of the Knighton Rural Sanitary District: £20 per annum; acreage 26,000; population 3000.

VERDON, W., M.R.C.S.E., has been appointed a District Surgeon to the Royal South London Dispensary, vice Duke, resigned.

WIDDAS, D., L.R.C.P.Ed., L.M., L.F.P. & S. Glas., has been appointed Medical Officer to the Perseverance Lodge of Odd Fellows, No. 1184, Thirsk, vice Swanson, resigned.

Births, Marriages, and Deaths.

BIRTHS.

BROWN.—On the 25th ult., at St. Bartholomew-road, Kentish-town, the wife of Andrew Brown, L.R.C.P.Ed. of a son.

GOOD.—On the 28th ult., at Wilton, Wilts, the wife of Joseph Good, L.R.C.P.Ed., of a daughter.

HILL.—On the 25th ult., at Guildford-street, Russell-square, the wife of John D. Hill, F.R.C.S.E., of a son.

KIRKMAN.—On the 4th inst., at Barming-heath, Maidstone, the wife of William P. Kirkman, M.D., of a daughter.

M'NALTY.—On the 8th inst., at Sutherland-gardens, W., the wife of G. W. M'Nalty, M.D., Surgeon-Major, of a daughter.

SMITH.—On the 25th ult., at Portugal-street, Grosvenor-square, the wife of Heywood Smith, M.D., of a son.

SMITH.—On the 13th ult., at Halifax, N.S., the wife of P. B. Smith, M.D., Surgeon-Major 87th Fusiliers, of a son.

MARRIAGES.

GOWERS—BAINES.—On the 6th inst., at Leeds, W. R. Gowers, M.D., of Queen Anne-street, Cavendish-square, to Mary, second daughter of Frederick Baines, Esq., of Westwood Lodge, Leeds.

LEE—MARTELL.—On the 31st ult., Timothy Wood Lee, M.R.C.S.E., of Little Milton, Oxon, to Margaret Jessie Martell, of Seaton, Devonshire.

WELCH—JUPP.—On the 7th inst., at Christ Church, South Hackney, Samuel Welch, M.R.C.S.E., of Hackney-road and Victoria-park-road, to Julia Sophia, daughter of John Jupp, Esq., of Victoria-park-road.

DEATHS.

ARCHER.—On the 5th inst., Francis Archer, M.R.C.S.E., of Crosby, Liverpool, aged 71.

BARKWAY.—On the 2nd inst., at Lavenham, R. E. E. Barkway, M.R.C.S.E., of Bungay, aged 42.

CALTHROP.—On the 22nd ult., H. J. Calthrop, M.R.C.S.E., of Gosberton, Lincolnshire.

DICKSON.—On the 30th ult., Thomas Dickson, F.R.C.S.E., F.R.C.S.Ed., of Buxton, aged 70.

FRYER.—On the 27th ult., Thomas Fryer, M.R.C.S.E., of Trowbridge, aged 79.

POWELL.—On the 30th ult., at Trinity-square (after a few days' illness), Dr. W. E. Powell. Deeply regretted.

SOUTHAM.—On the 1st inst., G. T. M. Southam, M.B., of Peterborough, aged 35.

TYLEY.—On the 28th of Feb., at South-bank, Wedmore, Somerset, Elizabeth, the dearly beloved wife of Richard Purnell Tyley, M.D., and only surviving daughter of Joseph Wallen, Esq., J.P., aged 38.

[N.B.—*A fee of 5s. is charged for the insertion of Notices of Births, Marriages, and Deaths.*]

Notes, Short Comments, and Answers to Correspondents.

VOLTA.

AT CAMNAGO, a township on the railway between Milan and Como, stands on an eminence the monument of Alessandro Volta, the inventor of the voltaic pile, who died in 1827. It consists of a round temple, reached by a flight of steps, with two symbolic statues on each side. In front is the bust of Volta by the sculptor Comolli, with two genii on each side—one in the act of crowning him, the other with his famous pile. Over the bust is a bas-relief, representing him showing his invention to the Institute of France. From the rotunda you descend to the crypt, where his remains repose. This tomb having been opened last October by Professor Mantegazza, of Florence, for the purpose of taking scientific measurements of the philosopher's skull, the coffin was found much decayed. On the 30th ult. a number of physicists, physicians, and *savants* assembled under a gallery before the monument to exhume the philosopher's remains, and then and there the cranium, which was well preserved, was scientifically examined by Professors Verga, Lombroso, and Cornalia. An account of their measurements will shortly be published. An urn of Carrara marble, in the form of an altar, having been made the new receptacle of his remains, orations were delivered on the occasion by Professor Cantoni, the Cavaliere Cesare Cantù, and other representatives of the various seats of learning and medical schools throughout Italy, commemorating the high moral and intellectual character of Alessandro Volta.

Mr. Lewis Davis.—We never recommend a particular physician or surgeon.

SELF-SUPPORTING HOSPITAL AT BRIGHTON.

To the Editor of THE LANCET.

SIR,—The letters of Sir Charles Trevelyan on "Hospitals for all Classes," and on a "Self-supporting Hospital at Brighton," open up a large subject, and one well worthy of careful thought. With but little dissent it appears to be the general opinion that the present almost indiscriminate mode of affording medical relief to the poor needs reform. Putting professional interests aside, that reform is needed in the interests of the poor who are not paupers; of the ratepayers, on whom the pecuniary burden of pauperism falls; and even in the interests of the nation and of the world. For indiscriminate charity has a direct tendency to pauperise its recipients, by inviting those to lean upon others who are capable of helping themselves; and it has a tendency, by insidiously undermining the independence of the people, to warp eventually the national character, which is essentially and distinctively self-reliant.

Sir Charles Trevelyan proposes to remedy this evil by the infusion of the provident and self-supporting principle into the system which now exists. He thinks that, by engrafting the provident scion on the gratuitous stock, both will flourish together; the virtues of the former acting as an antidote to the vices of the latter. Were the scion, as in arboriculture, likely to determine the character of the fruit, I should heartily hail the suggestion; but I believe that the principles of dependence and of providence are utterly incompatible with each other. Figs are not to be obtained from thistles. Sir Charles would go further: he would "popularise" our present hospitals, and try to bring to them additional classes of patients. This may, for aught I know, be safely done in other countries; but in our own I believe it would be replete with danger, and be more likely to bring down than to raise up. Gratuitous hospitals are, I submit, far too "popular" already. They were established and endowed, and are ostensibly maintained, for the relief and succour of the "sick and lame poor." They are, or should be, fundamentally and essentially charities. They hold, it is true, a place between the Poor-law system which exists, and the provident and self-supporting system which we seek to build up; but they have much greater affinity to the former than to the latter. They may, indeed, be considered as supplementary to the Poor-law system, and as destined, perhaps, ultimately to be appropriated by, or absorbed into, that system. On the contrary, the provident and self-supporting principle has nothing in common with, it is even antagonistic to, charity and dependency. It should always remain erect and self-reliant; and then I believe it will act, not as a questionable palliative, but as a radical cure of the abuses of medical charity.

At present great numbers, especially of the working classes, are, in case of accident or disease, driven to gratuitous hospitals, where they cannot afford, or their homes are not adapted for, home treatment, simply because these are the only hospitals which exist. The hospitals admit them for the same reason. But, if provident and self-supporting hospitals were available, the reason of the application for, as well as the excuse for affording, gratuitous relief would be removed, and the force of public opinion, if nothing else, would soon achieve the desired reform. What is really wanted is the restriction of the operations of our gratuitous institutions within the sphere of true charity, and the conversion of our *superfluous* free hospitals, dispensaries, infirmaries, lying-in charities, *et id genus omne*, into provident and self-supporting institutions, and the erection thereof where none would then exist.

I fully agree with Sir Charles Trevelyan that "hospitals for all classes," except the poor for whom provision is already made, are much needed; but they should be entirely free from all taint or *leaven* of charity; and then, with proper precautions and safeguards, they would tend to elevate, not to degrade. They should be, in short, simply and purely provident and self-supporting hospitals and sanatoria, such as we are seeking to establish at Brighton.

I am, Sir, faithfully yours,

Brighton, March 27th, 1875. JOSEPH STEPHENS, M.D.

Voyageur, (Liverpool.)—We believe that the whole of the £3000 required for the erection of a statue to the late Dr. Livingstone has already been subscribed by private individuals.

Hopeful.—If our correspondent will write to Messrs. Pollard, Brompton, he will obtain the desired information.

THE LYMPHATICS AND THE CAPILLARIES.

M. TARCHANOFF communicated to the Biological Society of Paris, Feb. 27th, 1875, his investigations respecting the communications alleged by Arnold to exist between the capillaries and the lymphatics in the shape of a network. The author contends that such network does not exist, and that it is artificially brought about by the injections. There is, in fact, diffusion of the colouring matter of the injected fluid, but no actual passage of the latter into pre-existing vessels.

Mr. A. G. Burness.—Our correspondent's paper shall receive attention.

PROFESSIONAL ETIQUETTE.

To the Editor of THE LANCET.

SIR,—The first sentence of Dr. Hassall's rejoinder contains an obvious misquotation of my reply to him. He says: "Dr. George Johnson now states that my being called in arose through a 'mistake' of Dr. Falls." On reference to my letter it will be seen that the mistake of which I spoke was the unfortunate delay to inform the patient of our choice of a medical attendant for him at Ventnor. For this mistake neither Dr. Falls nor I was responsible.

In a letter which I have received from Dr. Falls, and which I have that gentleman's permission to quote, he says: "If I may offer an opinion, I should say that the fact of my having written to Dr. Hassall respecting Mr. —— cannot be looked upon by him as final, as I only regard myself in the light of a second, and not a principal, medical adviser to Mr. ——, you and Mr. Plunkett having had him under your care from the first. Dr. Hassall would not have seen the patient had not Mr. Plunkett unluckily omitted to write."

If Dr. Hassall's demand for an explanation had been couched in less irate and minatory tones, it would have met with a prompt and complete response.

The family and the medical advisers of the patient were willing and anxious to explain and apologise for any seeming want of courtesy; but they were not to be deterred by angry threats from preferring the patient's welfare to a mistaken and perverted view of professional etiquette. Dr. Hassall's offensive insinuation as to a "new plea" and an "after-thought" needs no comment.

Dr. Hassall is a member of the Royal College of Physicians. Let him, then, bring his case before the President and Censors. The verdict of that tribunal, upon a full statement of all the circumstances, I shall most cheerfully accept.

I am, Sir, your obedient servant,

Savile-row, April 7th, 1875. GEORGE JOHNSON.

PRESSURE on our space obliges us to omit this week an article, in type, on the opening of the summer term of the Army Medical School at Netley. It will be published in our next.

THE MERRYWEATHER FUND.

Donations already acknowledged	£63	15	0
Major Roberts	0	10	0
Mrs. Roberts	0	10	0
Mrs. Morton	1	0	0
Benj. Miller, Esq., M.R.C.S., Denmark-hill, S.E....	0	10	6
D. De Berdt Hovell, Esq., F.R.C.S., Clapton, E....	1	0	0
Wm. Thorn, Esq., L.S.A., Harrow-road, W.	1	1	0
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Charles Braid, M.B., ...	1	1	0
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Dr. Lewis, 15, Ferdinand-strasse, Homburg... ...	1	1	0
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A Surgeon's Four Children	0	10	6
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Mrs. Jedson	5	0	0
Rev. J. Mallaby, Brighton...	3	0	0
Mrs. Bartlett, Peverell Court, Aylesbury	5	0	0
Mrs. Prescott, Dalton Grange, Wigan	10	0	0
Mrs. Sheldon, Ormskirk	5	0	0
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A Friend, Yorkshire	1	1	0
Linton B. Brunton, Esq., M.R.C.S., Limehouse...	1	1	0
Mrs. Lyon, Johnson Hall, Staffordshire	5	0	0
Henry G. Bull, M.D., Hereford	3	3	0
Rev. Henry Sach, Lancaster-road, W.	1	1	0
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Lily	0	10	6
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Fredk. Geo. Langmore, Esq., Westbourne-park ...	1	0	0
Dr. Frost, Ladbroke-square, W.	1	1	0
Charles Coates, M.D., F.R.C.P., Bath	2	2	0
W. M. K., Taunton	2	2	0
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Gloucester	1	0	0
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Miss Andrews	0	5	0
Alfred Harvey, M.D., Kilburn	0	10	0
John Copeland, L.R.C.P.E., South Hampstead ...	1	1	0
C. S. W.	1	0	0
Joseph Seaton, M.D., Sunbury...	3	3	0
	£148	16	0

Subscriptions should be sent to Dr. C. J. Hare, 57, Brook-street, Grosvenor-square, W.

BEYOND A JOKE.

IF the reports in the daily papers are to be trusted, a disgraceful hoax was practised on the medical men and dentists of Southport on the 1st inst. It is stated that on the morning of the day in question each practitioner received a letter requesting his attendance at ten o'clock at a certain house, either to see a patient or to join in a consultation. As the result, about a score of doctors drove to the spot indicated, only to find themselves called April fools for their pains. It is difficult to imagine what amusement there can have been in such a heartless joke, which has not even the merit of originality, and which must have entailed not only considerable loss of time and money on its direct subjects, but not improbably suffering and death on others. It needed no 1st of April to make a fool of the person or persons who planned the joke, and we trust that speedy detection and proper punishment will be meted out to the offender.

Mr. James Newell.—We would advise our correspondent to advertise.

THE BIRMINGHAM MEDICAL INSTITUTE AND HOMŒOPATHY.

To the Editor of THE LANCET.

SIR,—May I ask your permission to lay before your readers the following facts?

As you are doubtless aware, the troubles into which the Birmingham Medical Institute has been so recently plunged began when Dr. Heslop accepted from a professed homœopath a donation for the Institute, and induced his friends and colleagues to acquiesce in his high-handed action. But to show that Dr. Heslop was not always actuated by the kindly sentiments for the homœopaths that he now professes, I have extracted a portion of a letter addressed by him twenty-four years ago to the Editor of *Aris's Gazette* respecting homœopathy and its practitioners. After alleging that homœopathy and charlatanism are convertible terms, he says: "I am charged with an attempt to 'traduce' the body of which Dr. F—— is a member. I must ever do it. I am a legal guardian of the public health. Holding homœopathy to be a delusion, more ridiculous than any held in St. Luke's, more dangerous than the wildest maniac Mr. Bedlam has invented, it is a duty I owe to society to expose its folly and denounce its evils. I have diligently read some of the books of the new light, and can state that the travels of Captain Lemuel Gulliver, *splendide mendax* as he was, the theological jests of Father Newman, keenly as I relish them, are very heavy reading after the perusal of the wonders of Hahnemann—that Munchausen of medicine, the delightful *post hoc propter hoc* reasonings of Messrs. Sampson and Marsden, the transcendental tomfooleries of Black. Is there any joke in Swift equal to the assertion of the arch-homœopath, that the millionth of a grain of vegetable charcoal produces, among a hundred or two of other symptoms, 'a repugnance for butter'! I, too, have my advice for Dr. F—— (with whose University I am acquainted) and his friends. Beat a hasty retreat from a falling house. It is scarcely too late. The profession of medicine has pronounced, and its judgment is, that there are only two classes in a medical view—practitioners of medicine on the one hand; quacks on the other. Whatever grand name these may give themselves, quacks they remain in this absolute classification. But there is another equally cogent reason for their speedy retirement from their position. It is my conviction that at no very distant period the great triumvirate of human constitution-tinkers—hydropaths, homœopaths, and mesmerists—will do a sort of poetical justice to one another. The two former will perhaps throw 'one last cold water' on each other; while the mesmerist, by a desperate 'pass,' will extinguish himself and his fantastic German brethren in an endless sleep, as in the rare tradition of the mutual devouring of the Kilkenny cats, leaving *only* their 'tales' behind."

It was, of course, improbable that Dr. Heslop would be allowed to assume the character of innovator without being reminded of his previous opinion of homœopathy. Equal to the occasion, he said his convictions had lately undergone a material change, and that his present contrition had confirmed the statement "of the most eminent, one of the most charitable of men," that he (Dr. Heslop) would live to regret the letter he had written. At the annual meeting held on the 31st ultimo, he confessed that when he penned his diatribe against homœopathy he knew nothing of the subject, and that he was labouring under the influence of the prejudice of his teachers. But I submit that, by this confession, Dr. Heslop has forfeited his claim to be regarded as a guardian of the dignity of the profession, notwithstanding his boast that, "with grey hairs upon his head," he was willing to be held responsible for his own opinions. But if Dr. Heslop thought homœopathy so contemptible when his hair was black, and can say that "if the differences between himself and homœopathists were ten thousand times greater than they are" he should assent to their being members of the Institute, when his hair is grey, what, I may ask, are his opinions likely to be when he has no hair at all?

Your obedient servant,

April, 1875. JUVENIS.

T. H. J.—There are not, so far as we know, any medical appointments in connexion with the Customs either in China or Japan. For all other appointments abroad, inquire at the Foreign and Colonial Offices, Whitehall, S.W.

THE DAVEY DEFENCE FUND.

FURTHER contributions received on behalf of the above Fund:—

	£	s.	d.
S. Newington, Esq., Ticehurst	3	3	0
W. O. Jones, Esq., Cheshire	1	1	0
T. E. Owen, Esq., Plymouth	1	1	0
Dr. A. Playne, Maidenhead	1	1	0
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A. S. Bostock, Esq., Chichester	0	10	0
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Dr. W. Martin, Walkden	1	1	0
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Dr. S. Potter, Cullompton	1	1	0
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E. Robinson, Esq., Dukinfield	1	0	0
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Dr. C. Roberts, Uxbridge	1	1	0
Thos. Morris, Esq., Kent	2	2	0

Sigma.—If our correspondent will refer to some of our late numbers, he will find that his questions have already been answered.

Aliquis, (Brighton.)—Perhaps the ablest disciple of Gall was Dr. Fossati, who was born at Novara in 1786, and died in Paris at the close of last year. His views on cerebral physiology are well worth reading. They will be found in his "Questions Philosophiques traitées d'après les Principes de la Physiologie du Cerveau." Paris, 1869.

CHARITY VOTING REFORM ASSOCIATION: ROYAL MEDICAL BENEVOLENT COLLEGE.

To the Editor of THE LANCET.

SIR,—May I be allowed to call upon subscribers to all charitable institutions, and more especially medical men, and the numerous lay subscribers to the Royal Medical and Benevolent College. The subject of the election of beneficiaries to this College occupied so much of my thought six or eight years ago, that it may be guessed how rejoiced I now am that charity electioneering has within comparatively few months been taken up by a body of most energetic and philanthropic men in the establishment of a "Charity Voting Reform Association." Nothing can be more laudable and desired than such a bold and strong effort to assist the widow and fatherless to a means of alleviating their sorrows as well as their pockets, too many of which are but empty relics of their former position in life, may be, with a hard-working and loving husband.

As I before observed, the subject has long struck me as of such vital importance that as far back as the years alluded to I wrote upon it in your journal. At that time I even had interviews with the then Chairman of the Council of the Medical College, when I certainly had my hopes raised, though, alas! to be disappointed.

My chief object now in renewing the matter is to call upon every subscriber to give his or her aid in furthering such a laudable object, which may be done in more ways than one. The first of these is to sign and post to G. Seymour, Esq., at 30, Charing-cross, the blank form of memorial, which I have the gratification to know from him and Sir Chas. Trevelyan has been sent to every known governor or subscriber to the Royal Medical Benevolent College. I beg, therefore, to call upon *every* such member, and remind them of this most vital duty they owe to the widows of their departed brethren who may be left in more or less indigence, and who are wanting to obtain admission of their orphan children to the Foundation Scholarship.

I have long known that the traffic in votes on the days of election has been so patently cruel and uncertain that so far from the most indigent, helpless, and friendless candidate being elected, he has been driven to the wall by those who have the wealthiest and most influential friends to aid them—first, by the unsubstantial attachment of their names to the "card of recommendation," and next, by the pecuniary supply brought to bear on the day of election by the purchase of votes. I have the less hesitation in been thus outspoken, because, having been Hon. Secretary for many years, I have been at much trouble and some expense to forward the interests of the College, and I have frequently analysed these cards, and found that on an average from two- to three-fifths, and in some cases two-thirds, so recommending, are no subscribers at all; whereas, if I might digress, I would throw out a suggestion to the Council, that no name should be recognised that had not added some consideration to the Fund. Next, my knowledge of the dire traffic in votes is beyond contradiction on the days of election, as well as very damaging to any charity. Knowing these things, and seeing that *every* election must cost the needy mothers or their friends from £20 to £30, it requires but little comment to show how fearfully fallacious and cruel is the prevailing system of voting.

I could, Sir, if I have not already done so, tire both yourself and your readers with the numerous instances which have crossed my observation during the past twenty years at this College alone. Nay, for myself, I seem scarcely able to restrain my pen, which seems fain to run after the absurd fashion of the spirit-moving-table-turning-planchette mania of the present day among the more credulous part of the community. I will, however, try and cut it short by giving one or two instances of faulty election:—A case at Brighton in 1866. The widow of a medical man there was left with six or eight children, and she with as many hundreds a year income! Yet she and her friends wrote such a touching and plausible appeal to us in her card of canvass that I, for one, was induced to swerve from my usual course, and write in reply (aye, with tears in my eyes) to say I would give at least twenty votes. Soon after this I had the pleasure to dine at the College dinner in London, when the late noble founder—though not by blood perhaps, yet such he must be called as a philanthropist and benefactor, the late lamented John Propert, told me that this widow had the income referred to, and therefore could not, should not, be looked upon as a deserving recipient of the charity; yet be it known the boy *was* elected on his first application, and, if my recollection fails me not, by an excess of 4000 or 5000 votes above the second boy! If any of your readers will take the trouble to haul over the medical journals, he will find the case alluded to, and the remarks of the editors, "that it was the first case of the kind, and 'twas to be desired as the last," &c. But was it so? One case more will show, and I've done. At the very last election in May, two very opposite cases prevailed. One, the son of a deceased surgeon, leaving a widow with ten children, got in on his first application—some hundreds to spare. The second, son of a surgeon, leaving a widow with eight children, *failed* to get in on his *fifth* application, having polled about two-fifths as many votes in his *five* elections as the other did at his first application. Whence, then, I say, this disparity and failure but from the simple fact before referred to—friends and affluence of friends; the father of the one having been in large practice in London; the other, an obscure and hard-working man in a village in Hampshire! Surely there is something rotten here, that one can only exclaim, "O tempora, O mores."

Let me, in conclusion, reiterate my desire that all subscribers who have not already done so, look up these blank printed forms of memorial (they may be found in the waste-basket), sign them, put a halfpenny stamp on, and post them to our friend, G. Seymour, Esq., 30, Charing-cross, and let every heart be moved, to move heaven and earth to accomplish so desirable an object. And I would almost invite them to subscribe to *no* charity having such a misnomer attached to it; whereas I have the fullest conviction that under a more genial system of election subscriptions to every charity would increase.

Pardon me then, I pray you, Mr. Editor, for this encroachment on your time to read and space to publish, setting all down to my ardent zeal for the welfare of the *widow and the fatherless*.

I am, Sir, your obedient servant,

East Rudham, March 20th, 1875. FRED. MANBY.

KINMOND'S EFFERVESCING FLUID CITRATE OF MAGNESIA.

MR. KINMOND has devised a very ingenious bottle for this preparation, which he terms the "duplex" bottle. It contains two compartments, each having a separate mouth—the larger one containing the magnesia, and the smaller one holding the acid. We have tried the preparation, and find it very palatable and refreshing. The same manufacturer has just brought out a double strength fluid magnesia, which Dr. Attfield certifies to contain twice as much carbonate of magnesia as the fluid magnesia *of* the Pharmacopœia.

Dr. Vintras is thanked for his letter; but we cannot see that any good would accrue from its publication.

CROUP AND DIPHTHERIA.

To the Editor of THE LANCET.

SIR,—The discussion which has now through several weeks appeared in your pages on the relations of "croup and diphtheria" has been of singular interest, and the subject may possibly, in your opinion, allow of still further ventilation.

Your readers will probably grant with small hesitation that most of the late advances in medical knowledge have had place by the recognition of common causal conditions as lying at the root of many, on the surface, diverse manifestations; not by attempts to settle undecided questions by a counting of heads or a marshaling of opinions, which are formed often on insufficient data. Surely the appeal to the observers of the past is a course not a little to be deprecated, if their observations are to be cited as decisive in discussions of the present day. Let their reported cases and opinions have full weight as such, but yet as being of value for purposes of historical comparison more than for needs of present decision. It is hardly too strong a term to speak of the *unwisdom* of relying for differential diagnosis on the memories of the past, or on the records of those who did not possess the appliances of the present, and had not learned to trace to a common source symptoms having *inter se* very little in common.

Probably the divergence of opinion which your correspondents have evinced is mainly to be explained on the not unreasonable supposition that there are two distinct diseases which exhibit in their later stages very parallel conditions, and which tend to destroy life in the same way. In other words, there is a simple catarrhal laryngitis, which is comparatively of very frequent occurrence, is very intimately linked with cold and damp (e. g., residence in a damp, low-lying house, and exposure to cold easterly winds are the most ready excitants), and which is very amenable to early, well-directed treatment by warm baths, moist atmosphere, emetics, &c. &c., but which, if neglected in the first instance, or unchecked by appropriate measures, may certainly go on to a well-marked, more extensive laryngo-tracheitis, destroying life by suffocation, yet not attended by the appearances of a definite false membrane. Such cases I have certainly seen, where no appearance of coherent false membrane could be detected in the larynx and trachea on post-mortem examination. A typical instance of this very condition is given in Dr. Moxon's letter in your issue of March 13th.

There is again, too, a true membranous laryngitis, varying somewhat even in its early symptoms from the form of disease just noted, marked from its first commencement by the existence of distinct leathery exudation, and owing its origin to a special, probably aerial poison; not to the more usual morbid influences of cold or damp. If cases are seen sufficiently early in their course, and the fauces are examined with due attention, there will be found more or less decided similar exudation upon the mucous membrane of those parts. That such instances are occasionally sporadic proves nothing against their special diphtheritic character; while they are far more frequently seen in houses where several children are at the same time ill—some with the laryngeal form, others with some faucial diphtheria, and others it may well be with little of local mischief about the throat, but with decided indications of paralysis of certain nerves.

It were surely well if "croup" as an appellation were once and for all relegated to the description of the symptom of noisy metallic cough and breathing, and not used as the name of a special disease.

Personally I cannot but hold that the views adopted by Sir W. Jenner and Dr. Geo. Johnson are in our present state of knowledge to be accepted as almost literally true. The weight of evidence is in favour of their validity, and I, for one, have long believed that membranous croup and diphtheritic laryngitis with exudation of false membrane are one and the same disease.

In this expression of opinion I am directly at issue with Dr. Walters (THE LANCET, March 30th); nor can I accept without earnest protest his supposition that croup is a disease of the country, diphtheritic tracheitis one of towns. There is surely no such approach to a limitation of morbid processes. The insanitary conditions which provoke an outburst of diphtheria are unhappily as frequent in country houses and villages as in towns, and a residence in the open country confers no immunity upon its tenants if they decide to ignore the simplest requirements of sanitary well-being. Had Dr. Walters written "*catarrhal laryngitis* and diphtheritic tracheitis are essentially different forms of disease," I would yield a ready assent to his formula; but in its present form I cannot but think that his conclusions are open to very serious question.

I am, Sir, your obedient servant,
WILLIAM NEWMAN, M.D. Lond., F.R.C.S. Eng.

Stamford, March 23rd, 1875.

To the Editor of THE LANCET.

SIR,—The question as to the identity of membranous croup and diphtheria is worthy of the attention that has recently been directed to it; but I confess I have some difficulty in understanding how the two diseases *can* be ranged under the same category.

Between thirty and forty years ago I saw a large number of cases of membranous croup, and its characters were of the most pronounced kind: 1st. The type was sthenic. 2nd. The exciting cause was atmospheric—i. e., damp and cold. 3rd. The treatment most successful (in my hands) was antiphlogistic—viz., tartar emetic in frequent and nauseating doses. 4th. It was non-contagious. I never saw two cases in one family either occurring simultaneously or in immediate succession, and no precautions or isolation were adopted or necessary. 5th. It was never followed by paralysis or other consequences affecting the nervous system. 6th. It was limited to a certain age, never in my experience occurring after twelve years of age. 7th. The membrane, as ascertained by post-mortem examinations (which were sufficiently frequent) was *tough*, *tense*, and *light coloured*, and was strictly limited to the laryngo-tracheal tract, at least in its upper part, though it not seldom passed downwards into the bronchi in the form of plastic bronchitis. The disease never invaded the pharynx, fauces, or nose.

All who have seen much of diphtheria cannot fail to remark how totally different pronounced cases of it are:—1st. The type is asthenic. 2nd. The exciting cause is septic. 3rd. The treatment is antiseptic, nourishing, and stimulating. 4th. It is markedly contagious, and, unless the most stringent precautions are adopted, attacks and proves fatal to other members of the family or those in direct contact with it. 5th. Nervous symptoms, especially paralysis, are not rare consequences—indeed are always to be feared. 6th. It has no respect for age. 7th. The membrane is ash-grey, friable, in many cases almost pultaceous, and generally soft. It is seldom limited to the laryngo-tracheal tract; but, as frequently as not, extends to the pharynx, fauces, and nose, and very frequently *begins* there (a course utterly unknown in membranous croup).

Typhus and typhoid fevers are recognised now by sufficiently distinctive characters; but they are, in my opinion, not more or indeed so distinct as are membranous croup and diphtheria.

Your obedient servant,
Edinburgh, March 20th, 1875. WILLIAM CUMMING.

A. M. D.—We are compelled to postpone the insertion of our remarks on the numerous pamphlets that have been forwarded us regarding the Army Medical Department.

AN APPEAL.

To the Editor of THE LANCET.

SIR,—An appeal is earnestly made on behalf of the widow of the late George Darby Dermott, the well-known lecturer at the School of Anatomy in Charlotte-street, Bloomsbury.

Mrs. Dermott is 73 years of age, and is suffering from defective sight and many bodily infirmities, which require more attendance and comforts than the small income she possesses can procure. It is, therefore, hoped that by the liberality of the profession, and especially of the friends of her late talented husband, a sum may be raised which will be sufficient to place her for the brief remainder of her life in more comfortable circumstances.

Thirty years have now passed away since the death of Mr. Dermott; but, doubtless, there are many of his pupils still living who, on noticing this appeal, will willingly add their contributions. The following sums have been promised:—

Sir W. Jenner, Bart.	£5	5	0
Sir H. Thompson	5	5	0
Dr. Dow, Bayswater	1	1	0
Dr. Geo. Ross, Bloomsbury	2	2	0
J. Roche Lynch, Esq., Notting-hill...	2	2	0
M. B. C.	1	1	0

Contributions will be thankfully received and acknowledged by Dr. Geo. Ross, 11, Hart-street, Bloomsbury; or by the undersigned,

J. ROCHE LYNCH,
April 5th, 1873. 41, Chepstow-villas, Notting-hill, W.

Surgeon-Major Franklyn.—The charge appears to us to be in excess of the usual terms for such services. It is difficult to advise our correspondent what to do in the matter. The claim, of course, may be disputed, and carried into court.

DEATH BY STARVATION IN PRISON.

To the Editor of THE LANCET.

SIR,—I hope the recent unfortunate death in the Jersey Gaol will lead prison authorities to reconsider the whole question of the low diet usually given for a portion of a prisoner's sentence, as being a system directly tending to produce similar fatal results to the one in question, and to foster the outbreak of latent disease, thus to endanger, if not sacrifice, many lives.

Under the present plan, by which the "bread-and-water" punishment is inflicted immediately on commencing the term of imprisonment, the danger of insidious disease proving fatal is the greatest; for the prison surgeon is in most cases unaware of the special tendencies to disease among the prisoners newly placed under his care, and the examination made on the prisoner's admission is usually a cursory one, unless complaint of illness is made, or something in the criminal's aspect excites inquiry. It is, therefore, quite easy for a tuberculous prisoner, with a slight cold on the chest, to be unwittingly placed on bread-and-water diet in a cold cell, and to succumb quickly under scrofulous pneumonia.

A case of this kind came under my own observation at the commencement of my duties as a prison surgeon, when the patient, though removed to the infirmary as soon as the nature of the disease was apparent to me, and allowed generous diet, rapidly sank (a circumstance which has caused me ever since to feel a great detestation of the system); and I should imagine similar cases are not extremely rare. Two remedies may be suggested: either never to inflict low diet till the surgeon has signed a certificate, after special examination, that the prisoner is suffering from no disease likely to render such a diet dangerous to life; or, which would be far better, to abolish the low-diet scales altogether, but to withhold each meal until a certain amount of useful labour had been performed, and to give food of improved quality if the conduct and diligence were above the average.

Your obedient servant,
THOS. WEMYSS BOGG,
Louth, March 30th, 1875. Late Surgeon, Louth Prison.

MEDICAL ETHICS.

To the Editor of THE LANCET.

SIR,—As I am the person referred to in Mr. Mitchell Wilson's letter in this day's LANCET as having shown "an utter want of professional courtesy," I beg to explain that his statements are entirely at variance with the facts, which are as follows:—

I was sent for to see the patient on March 6th, but did not call till the following day, when I examined her, and advised an immediate operation. As the patient and her family consented, I made all necessary arrangements. Up to that moment there was no intimation given, nor was I aware, of Mr. Wilson's attendance. It is utterly untrue that I called to see another patient in the same house on that day, or that I was asked if I agreed with Mr. Wilson's opinion, or that I offered my services for nothing. On the contrary, a fee was named by the family, and I certainly expect to be paid.

I purposely confine my statement to facts within my own knowledge; but I have forwarded Mr. Wilson's letter to the friends of the patient, and I beg you will insert their statement (should they send one) as well as this letter in your next.

Your obedient servant,
Chatteris, April 3rd, 1875. MARTIN O'CONNOR, L.K.Q.C.P.I., &c.

DISLOCATION OF PATELLA.

To the Editor of THE LANCET.

SIR,—Henry A——, aged twenty-one, moulder, employed at Stanley Iron Works, was on the 27th March accidentally struck by an iron pipe, causing the left patella to be completely turned round, the outer edge lying immediately under the skin, and the inner edge fixed and immovable, as if screwed down on the femur. Reduction was attempted without success. The patient was then placed fully under the influence of chloroform, and Dr. Booth succeeded, after a little manipulation, in reducing it, the welcome sound of the snap being distinctly heard, showing that the patella was again in its place.

The knee presented a singular appearance on admission, and as it is rather a rare variety of this dislocation, you may think it worth placing on record.

The late Professor Syme used to say that there was almost nothing so satisfactory as a successful surgical manipulation or operation, and this is quite true, as exemplified in this case, as well as in others of far greater importance.

I am, Sir, yours faithfully,

Chesterfield Hospital, March 19th, 1875. JOHN ROSE, M.D.

COMMUNICATIONS not noticed in the current number will receive attention in our issue of the ensuing week.

COMMUNICATIONS, LETTERS, &c., have been received from—Dr. Greenhow, London; Dr. George Johnson, London; Mr. Haynes Walton, London; Dr. Hughlings Jackson, London; Mr. Oliver Pemberton, Birmingham; Dr. Dobell, London; Dr. Whitelaw; Dr. Fleming, Glasgow; Col. Thomson, Aldershot; Mr. Nunneley, Leeds; Dr. Vintras, London; Dr. Crowther, St. Leonards; Surgeon-General Dr. Maclean; Mr. Thos. Smith, London; Mr. Poles, London; Mr. Reilly, London; Mr. A. Kershaw, Farnworth; Dr. Steele, Montrose; Mr. L. Davis; Mr. Poole, London; Mr. De Styrap; Mr. S. Brown; Dr. Taaffe, Brighton; Dr. Hugo, Senftleben; Mr. Spencer, London; Mr. Whitbread, M.P.; Dr. Chatteris, Glasgow; Mr. Bramwell, Newcastle-on-Tyne; Mr. Groves, London; Lord Enfield; Dr. Coupland, London; Mr. Thomas, Sandwich; Dr. C. Fox; Mr. Roche Lynch, London; Mr. Ellis, Newcastle-on-Tyne; Mr. Lowsley, Reading; Mr. A. Burness, London; Surgeon-Major Hunter, Bombay; Dr. Heywood Smith, London; Mr. Williams, London; Mr. Greenwood, Penryn; Mr. Cullingworth, Manchester; Mr. O'Connor, Chatteris; Mr. Newell, Clonmel; Dr. H. J. Ilott, Croydon; Dr. Whitmarsh, Hounslow; Mr. Eddison, Leeds; Mr. Young, Rugeley; Mr. Larmuth, Manchester; Messrs. Fox and Co., Manchester; Dr. Mason, Sheffield; Mr. Raye, Ledbury; Dr. Oliver, Halifax, N.S.; Mr. Smith, Winchester; Dr. Long, Hackney; Mr. Lancaster, Bradford; Mr. Wason, London; Mr. Dayson, Leeds; Mr. Hughes; Mr. M'Kellar, London; Mr. Forrest, Manchester; Mr. Thomas, Shirehampton; Mr. Reid, Lisburn; Mr. Bindley, Brighouse; Mr. Duncan, London; Dr. Kirkman, Barming Heath; Mr. Godfrey, Northampton; Surgeon-Major Franklyn, Charlton; Mr. Leiterschosen, Homburg; Mr. Burroughs, Weston Toyland; Surgeon-Major Ringer, Sitapur, India; Mr. Gill, London; Mr. Greenwood, Liverpool; Dr. Reed, Reading; Mr. Coxon, Burton-on-Trent; Dr. Lawson, Wakefield; Dr. Beale, London; Mr. Burdett, Greenwich; Dr. Haughton, Norwood; Rev. M. A. Gathercole, Chatteris; Mr. C. M. Gurney, London; Mr. Austen, London; Dr. Hay, Hull; Mr. Whalley, Bradford; Dr. Taylor, Nottingham; Dr. Jagielski, London; Mr. Armstrong, Newcastle-on-Tyne; Mr. Bennett, Haverstock-hill; Dr. Alexander, Bradford; Dr. Hefferman, Spennymoor; Messrs. Corbyn and Co., London; Mr. Lloyd, Denbigh; Mr. Aylward, Hastings; Mr. Macintyre, Barbreak; Messrs. Cook and Son, London; Mr. Sergeant, London; Mr. Unsworth, Liverpool; Dr. Hulme, Wigston Magna; Dr. Stephenson, Fawley; Dr. Cooke, London; Dr. Rae, Blackburn; Dr. Burder, Bristol; Mr. Robjohn, London; Mr. Crawford, Walker-on-Tyne; Mr. Toller, Barnstaple; Mr. Elwen, Dover; Mr. Forbes, Glasgow; Mr. Day, Edinburgh; Registrar-General, Edinburgh; Hopeful; A Taker-in of THE LANCET; Zero; A Ship Surgeon; N. G., Paris; H.; T. R.; M. H. W.; A Country Doctor; G. H., New York; J. W. D.; Sigma; J. W. L.; A. M. D.; A. B. C.; A Londoner; &c. &c.

LETTERS, each *with enclosure*, are also acknowledged from—Mr. Stanton, Chorley; Mr. Byass, Abingdon; Mr. Pilgrim, Dawley; Mr. Walker, Crick; Mr. Duke, Buckingham; Mr. Clarke, Framwellgate Moor; Mr. Walshaw, Halifax; Mr. James, Abergavenny; Mr. Marsdin, Eastbourne; Mr. MacGill, Littleborough; Mr. Jones, Ty-Mawr; Mr. Callant, Bridgnorth; Mr. Rayner, Warwick; Mr. Shaw, Attercliffe; Mr. Berry, Newcastle-on-Tyne; Mr. Stott, Haslingden; Dr. Barkley, Frizington; Mr. Campbell, Staunton; Mr. Collier, Carbrook; Mr. Wall, Wigan; Dr. Tyley, Wedmore; Mr. Wood, Little Milton; Mr. Lilley, Glasgow; Mr. Currie, Thetford; Mr. Rigden, London; Dr. Marsden, London; Mr. Prod, Esher; Mr. Duke, Dublin; Mr. Blacket, Newbury; Mr. Platt, Oldham; Mr. M'Ilwraith, Girvan; Mr. Aylward, Hastings; Mr. Dixon, Leicester; Mr. Mortimer, Malta; Mr. Aylen, Bhandora; Mr. Wiltshire, Sheffield; Mr. Griffith, St. Helens; Dr. Walker, Hanley; Mr. Mackenzie, Normanton; Mr. Fairles, Montgomery; Dr. Stewart, Barming Heath; Mr. Walker, Corwen; Mr. Rigg, Petersfield; Mr. Knott, Worcester; Dr. Watson, Wortley; Mr. Gale, Bristol; Mr. Sharp, Truro; Mr. Cawthra, Bradford; Messrs. J. and R. Kemp, London; Mr. Allen, Eastbourne; Mr. Marshall, Leeds; Mr. Collingwood, Middleton-in-Teesdale; Dr. Lang, South Kensington; Mr. Lithgow, Seaton Carew; Mr. Snow, Dorchester; Dr. Richmond, Northallerton; Mr. Sutcliff, London; J. S. H., Bristol; G. B., London; Medicus, Hunslet; M. D., London; Medicus, Glasgow.

Newcastle Daily Chronicle, Manchester Guardian, Western Morning News, Surrey Advertiser, Huddersfield Chronicle, Liverpool Post, Cork Constitution, Derby and Chesterfield Reporter, East London Observer, Western Mercury, Hackney and Kingsland Gazette, North British Daily Mail, Animal World, and *Hereford Journal* have been received.

METEOROLOGICAL READINGS

(*Taken by Steward's Instruments*).

THE LANCET OFFICE, APRIL 8TH, 1875.

Date.	Barometer reduced to Sea Level, and 32° F.	Direction of Wind.	Wet Bulb.	Dry Bulb	Solar Radiation in Vacuo	Max. Temp. Shade	Min. Temp.	Rain-fall.	Remarks at 9 A.M.
April 2	30·40	N.W.	42	46	...	56	45	...	Overcast
„ 3	30·03	W.	42	45	...	54	42	...	Overcast
„ 4	29·69	W.	46	49	...	59	44	0·09	Cloudy
„ 5	29·30	S.W.	45	47	...	56	46	...	Overcast
„ 6	29·44	W.	44	47	...	56	42	...	Cloudy
„ 7	29·35	W.	39	40	...	52	39	0·23	Cloudy
„ 8	29·39	W.	40	42	...	...	38	0·09	Cloudy

Medical Diary for the ensuing Week.

Monday, April 12.

ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M. each day, and at the same hour.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M. each day, and at the same hour.
ST. MARK'S HOSPITAL.—Operations, 9 A.M. and 2 P.M.
METROPOLITAN FREE HOSPITAL.—Operations, 2 P.M.
MEDICAL SOCIETY OF LONDON.—8 P.M. Mr. Churton: A Case of Accumulation in the Ascending Colon, followed by Spasm of Larynx, in an Infant." — Dr. Edwards Crisp, "On Croup and Diphtheria, with recent illustrative Cases." — Staff Surgeon L. H. J. Hayne (communicated by the Hon. Sec.): "An Account of an Epidemic of Enteric Fever which occurred on board H.M.S. *Doris* in the West Indies in 1873."

Tuesday, April 13.

GUY'S HOSPITAL.—Operations, 1½ P.M., and on Friday at the same hour.
WESTMINSTER HOSPITAL.—Operations, 2 P.M.
NATIONAL ORTHOPÆDIC HOSPITAL.—Operations, 2 P.M.
WEST LONDON HOSPITAL.—Operations, 3 P.M.
ROYAL MEDICAL AND CHIRURGICAL SOCIETY.—8 P.M. Ballot.—8½ P.M. Mr. Acton, "On the Comparative Prevalence and Severity of Syphilis among Troops in London, Paris, and Brussels."

Wednesday, April 14.

MIDDLESEX HOSPITAL.—Operations, 1 P.M.
ST. MARY'S HOSPITAL.—Operations, 1½ P.M.
ST. BARTHOLOMEW'S HOSPITAL.—Operations, 1½ P.M., and on Saturday at the same hour.
ST. THOMAS'S HOSPITAL.—Operations, 1½ P.M., and on Saturday at the same hour.
KING'S COLLEGE HOSPITAL.—Operations, 2 P.M., and on Saturday at 1½ P.M.
GREAT NORTHERN HOSPITAL.—Operations, 2 P.M.
UNIVERSITY COLLEGE HOSPITAL.—Operations, 2 P.M., and on Saturday at the same hour.
LONDON HOSPITAL.—Operations, 2 P.M.
SAMARITAN FREE HOSPITAL FOR WOMEN AND CHILDREN.—Operations, 2½ P.M.
HUNTERIAN SOCIETY.—7½ P.M. Council Meeting.—8 P.M. General Meeting.—Dr. Pye-Smith will introduce the subject of Hodgkin's Disease (Leucæmia).

Thursday, April 15.

ST. GEORGE'S HOSPITAL.—Operations, 1 P.M.
ROYAL ORTHOPÆDIC HOSPITAL.—Operations, 2 P.M.
CENTRAL LONDON OPHTHALMIC HOSPITAL.—Operations, 2 P.M., and on Friday at the same hour.
HARVEIAN SOCIETY OF LONDON.—8 P.M. Mr. Geo. Field: "Cases in Aural Surgery."

Friday, April 16.

ST. GEORGE'S HOSPITAL.—Ophthalmic Operations, 1½ P.M.
ROYAL SOUTH LONDON OPHTHALMIC HOSPITAL.—Operations, 2 P.M.
MEDICAL MICROSCOPICAL SOCIETY.—8 P.M.

Saturday, April 17.

ROYAL FREE HOSPITAL.—Operations, 9 A.M. and 2 P.M.
CHARING-CROSS HOSPITAL.—Operations, 2 P.M.

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